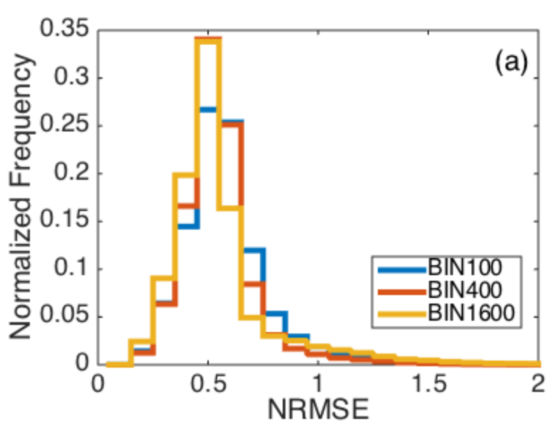
We would like to thank all three referees for taking the time to review our manuscript. All referees felt that the manuscript was confusing, and we have substantially revised the manuscript to address these concerns, including a modified methodology, new figures, and clearer discussion. The basic conclusions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

Anonymous Referee #1
Received and published: 11 March 2016
Review of "The role of the size distribution shape in determining differences between condensation rates in bin and bulk microphysical schemes" by Igel and van den Heever. This is a confusing manuscript of very little significance for modeling of atmospheric clouds in my opinion. I have several general and many specific comments that need to be addressed before the manuscript is accepted in ACP. Because of little significance, I do not want to re-review the revised manuscript. The handling Editor should be able to judge if my comments are appropriately addressed.
We have addressed the general and specific comments below. Here we would like to address the comment regarding significance. Bin and bulk microphysics schemes take fundamentally different approaches to describing cloud size distributions. Because bin schemes are much more expensive computationally, but otherwise generally believed to be superior, there is a need to understand how bulk schemes can be improved based on the behavior of bin schemes. We believe that this paper makes a significant contribution towards identifying the important and unimportant differences between the two schemes. Specifically, our results suggest that an assumed gamma size distribution by bulk schemes does NOT induce a large degree of error *if the correct value of the shape parameter can be known*. We feel that this is a significant conclusion, and one that is not obvious or expected. Given the multiple questions raised by the referee about the inappropriateness of the gamma distribution and multimodality, they do not seem to think that this is an obvious or expected result either.

General comments.
1. I found the whole logic behind this paper (including the title) confusing. Unless cloud droplets are very small (in which case surface tension, solute, and molecular effects need to be considered) or they are large (tens of microns, in which case ventilation effects are important), the condensation rate for a given supersaturation depends on the integral radius alone, that is, on the integral of the product of the droplet concentration and the droplet radius. (This is incorrectly called "integrated radius" in the manuscript). The reference to the spectral shape is confusing because the condensation rate depends on the spectral shape indirectly. For instance, if the spectrum is symmetric, the spectral width is irrelevant because in such case the integral radius is independent of the width. Of course the gamma distribution is asymmetric. The difference between the condensation rate as given by Eqs. (2) and (3) is that the assumed droplet distribution is analytically integrated in (2) in contrast to the approximation of the integral by the sum over finite number of bins in (3). So the difference may come from the assumed shape of the spectrum in the bulk scheme (in contrast to freely-evolving shape in the bin scheme), but it may also come from an inaccurate representation of the spectrum with a small number of bins (note that the number of bins is rather low in the Khain's scheme).

We agree with the reviewer that the impact of the spectral width will depend on the asymmetry of the size distribution, and will have no impact in the case of a symmetric distribution. What we find powerful is that when we assume a specific distribution function – specifically, the gamma distribution function, which as noted by the reviewer is asymmetric – the shape parameter (which quantifies the spectral width) *IS* able to account for much of the discrepancies in the condensation/evaporation rate between the two schemes, despite all of the potential pitfalls that the reviewer mentions such as the potential inaccurate representation of the droplet spectrum, or the potential for multi-modal or non-gamma-like distributions. We too would have expected such issues to be more important and thus we think that the conclusions we draw are important and worthy of publication.

2. The gamma size distribution is perhaps a sensible representation of possible droplet spectral shapes, but it is by no means ideal. Realistic situations involve various shapes, including often-observed bimodal spectra and occasional multi-modal. Such spectra cannot be represented by the gamma distribution, but can be simulated by the bin scheme. So how important are the spectral shape differences simulated in the current study? Are the differences in the condensation rate correlated with the asymmetry and/or multimodality of the spectra simulated by the bin scheme?
We agree that the gamma size distribution is by no means ideal. We calculated the normalized root mean square error for each of the fitted gamma distributions from the bin simulations. A value of 1 indicates that the fit is better than a straight line. The NRMSE's are generally less than 1 and indicate that most of the time the gamma distribution has some skill in approximating the simulated size distribution. (We recognize that this doesn't necessarily mean that a different distribution wouldn't be better.) We have also attempted to assess how these cloud droplet size distributions with poor fits impact the comparison with the bulk scheme condensation and evaporation rates. There is an entirely new section of the manuscript dedicated to this topic. In summary, we do not find that the non-gamma-like DSDs severely deteriorate the comparison of the rates. This is both because they do not occur very frequently, and because even with only a mediocre fit, the best-fit shape parameters still seem to be able to account for much of the difference between the bulk and bin scheme condensation and evaporation rates. It is certainly not perfect, but it is an improvement.



Above: Distribution of NRMSE values from the three bin simulations.

3. I think differences shown in the paper need to be put in the context of bulk cloud properties to see if they play any role. The fact that condensation rates differ for given supersaturation and integral radius tells me little because of the interactive nature of the condensation. In a real situation, a different condensation rate modifies the super-saturation and the overall effect might be insignificant. In other words, one needs to see the change of the supersaturation for a modified condensation rate, and not the condensation rate for a given supersaturation. Think quasi-equilibrium supersaturation. Does the simulation applying one formulation differ significantly from the other? If not, then why worry?
The short answer is that yes, changing the value of the shape parameter in a bulk simulation can have large impacts on the cloud properties. These changes are discussed in detail in Igel et al. 2016b (accepted pending revision). We know more generally that bin and bulk schemes (or more generally any two microphysics schemes) often simulate very different cloud properties and we have very little understanding about why this is the case. Even if differences in the condensation and evaporation formulations do not turn out to cause the simulations to be different from one another, this would be worth knowing since we do not know which microphysical processes contribute most to the differences. This study is just one step towards understanding the behavior of these different schemes.

In regards to quasi-equilibrium supersaturation, we see the referee's point that it may not matter how we get to equilibrium if the equilibrium state itself is the same regardless of the scheme. We also agree that analyzing the change in supersaturation in a similar way as we have done for the condensation and evaporation rates could be interesting, but we are not sure what additional information that would give. We have found that the mean supersaturation can vary by 0.2-0.4% depending on the shape parameter used in the bulk simulations which suggests that the quasi-equilibrium state is not the same. Furthermore, the concept of quasi-equilibrium only applies to the cloud core. By our estimate, at most 25% of the cloudy points are in the cloud core (this is the percent of cloudy points that are both supersaturated and in an updraft). Given that 75% of cloudy points not in the cloud core, and that the quasi-equilibrium is impacted, we think that the understanding how the condensation and evaporation rates differ between the schemes is important.

Specific comments
1. Abstract. L. 14: I do not consider the approach used in the paper particularly novel.
It is not an approach we have seen others use to compare microphysics schemes.

L. 16: "Integrated diameter" should be "integral diameter" (and in many paces in the text).
We have revised the analysis such that this term is no longer used at all.

L. 23: The fact that the maximum deviation may reach 50% tells me little. What about the mean or median inside each bin? And what impact does it have on cloud properties? See 3 above.
In the revised manuscript, we discuss in detail the means of the bins. The impact of a

change in the shape parameter on cloud properties is discussed in Igel et al. 2016b (accepted pending revision).

2. L. 71/72: Was the change in Morrison and Grabowski related to condensation or to the drizzle formation? I think the latter. If so, this is really not relevant to the subject matter of this paper.
Morrison and Grabowski do not discuss the reasons for why a change in the N-ν relationships changed the cloud water path.

3. Section 2, modeling setup. I am curious why such a complex modeling setup was chosen, with interactive land-surface model and radiation. There exist much simpler cases (like BOMEX or RICO for the maritime environment or diurnal cycle of shallow convection over the ARM SGP by Brown et al. QJ). A simpler case eliminates feedbacks between clouds and other processes that can make the simulations with different microphysics schemes to diverge more rapidly. The two simulations diverge eventually (the butterfly effect), correct? Moreover, if such a simpler and already documented case is used, the simulation can be compared with results from other models and give more credibility to RAMS results.
These simulations were used for additional studies (Igel et al. 2016a, b, accepted pending revision). The details are provided for completeness, although we agree that a simpler set-up could have been used.

4. Walko et al (2000) is actually two papers, 2000a and 2000b. However, (2) is not presented in Walko et al. so a different reference is needed. Moreover, Walko et al. paper starts with the invariant temperature proposed by Tripoli and Cotton. How is this relevant for a scheme that predicts the supersaturation? Something is not correct here. Also, RAMS use to have a much better bin microphysics (when Stevens and Feingold were at CSU), without ice, but with a significantly better representation of warm-rain processes (double-moment). One can enhance this study using that bin scheme in the comparison as well (just a comment).
Yes, there are two Walko et al (2000) studies and we neglected to indicate which we were referring to. It is 2000b. We are aware that Eq. 2 is not in Walko et al. (2000b), which is why we have explicitly stated that Eq. 2 is a rearranged and simplified version of Walko's Eq. 6. There exists no reference for Eq. 2.

We assume that the reviewer is asking about the use of the ice-liquid temperature. This temperature is invariant for internal water phase changes and does not rely on any assumptions about saturation. Thus, it is perfectly suitable for use in a condensation schemes that allows for supersaturation.

The former bin scheme in RAMS is not available in the standard code and thus was not available for comparison.

5. L. 111/112. This is not correct. Condensation in the bin scheme results in the shift of droplets from one bin to the next one.
True, the end result is that droplets shift bins. However, this shifting is only done after the

calculation of condensation rates. The shifting of droplets is done in such a way as to conserve the new total mass, total number, and total reflectivity of the droplet population.

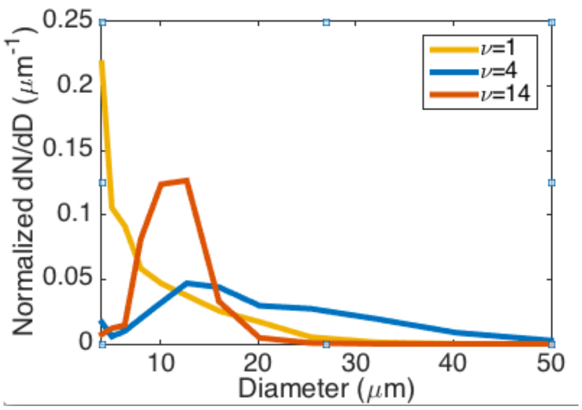
6. L. 129/130. If clouds reach the model top, the domain is too shallow, even a few hours earlier. This is bad experimental design.
At the final time included in our analysis, the maximum cloud top is about 750m from the model top. This may indeed be too close, but based on examination of the vertical velocity vertical profiles, we believe that the clouds may be too close to the top for at most only the last hour. However, since we are not examining cloud macrophysical properties or evolution, but rather only instantaneous condensation and evaporation rates, the location of the clouds relative to the model top is not at all an issue for our analysis.

7. L. 143, "aerosol surface deposition". What is that? Please explain.
We mean dry deposition or gravitational settling. This has been clarified within the manuscript.

8. L. 148/155. How many bin are used in the bin code? Are results sensitive to the number of bins used? What is the shape parameter value for the bulk scheme?
The SBM uses 33 mass-doubling bins. We cannot easily test the sensitivity of the results to the number of bins used without significantly restructuring the code. The shape parameter for the bulk scheme was originally 4. We now also test values of 2 and 7.

9. L. 173 and several other places. What is "saturation ratio"? Please define.
It is now defined. It is the same as relative humidity except not multiplied by 100%.

10. Section 4.2. It is unclear to me why one might expect that a bin scheme with a small number of bins can provide a useful estimate of the shape parameter. This is clearly impossible for bimodal and multimodal spectra. At least a comment on this would be appropriate.
We believe that 33 bins are more than sufficient to capture bimodal distributions and to find a shape parameter for each mode. The following figure shows three example distributions simulated by the bin scheme. This figure shows only the first 15 bins, and the legend indicates the best-fit shape parameter. The scheme can clearly produce droplet size distributions that have distinct widths and that can be well-characterized by a shape parameter. Similarly variations in behavior can be captured with the remaining 18 bins, which are for raindrop-sized drops. Thus with 33 total bins, the bimodal nature of the cloud-rain size distribution is can be simulated.



11. L. 316 and abstract: It is obviously the shape of the spectrum (prescribed in the bin scheme and evolving freely in the bin scheme) that is responsible for the difference between the two schemes. So this conclusion is kind of obvious. Please see my general comment 1.

See also our response to comment 1.

12. The appendix provides very little useful information and can be removed from the manuscript.

We agree that the appendix is not particularly relevant to the study, however we choose to keep it to document the implementation of the SBM into RAMS.

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.*