

# ***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***

## **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.

General comments.

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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).

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?

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-

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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?

#### Specific comments

1. Abstract. L. 14: I do not consider the approach used in the paper particularly novel. L. 16: “Integrated diameter” should be “integral diameter” (and in many places in the text). 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.

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.

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.

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.

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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).

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.

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.

7. L. 143, “aerosol surface deposition”. What is that? Please explain.

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?

9. L. 173 and several other places. What is “saturation ratio”? Please define.

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.

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.

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

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Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-64, 2016.

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