Review of study "The role of the Gamma function shape parameter in determining differences between condensation rates in bin and bulk microphysics schemes" authored by A. Igel, and S. van den Heever.

The rates of condensation and evaporation obtained in simulations with bin and bulk microphysical schemes are compared in simulations of non-precipitating shallow cumulus clouds. It is shown that the difference between the rates is largely because of non-optimum choice of shape parameter in the Gamma distribution used in the bulk-scheme.

Corrections in the rates of condensation and evaporation in the bulk -scheme are introduced to get better agreement with those in the bin-scheme.

The topic of the paper is important. The calibration of bulk-parameterization schemes using bin-schemes as benchmark is an important way to improve bulk-schemes and the skill of cloud-resolving models. At the same time I have very serious remarks to the current study. The paper cannon be published in the present way. *I would recommend to discuss the possibility of publication after major revision.*

The comments and remarks are the following.

1. General comment: the paper is written in a very unclear way. It is difficult to follow the conclusions and statements of the authors. The paper contains a lot of complicated discussions, assumptions, and conclusions which are not illustrated either by formulas or by figures.

2. line 73. Is it possible to plot in fig 2 (or in a separate figure) the values of shape parameters that can be derived from the bin scheme used?

3.line 114. Does the expression (2) mean that supersaturation is assumed constant during one time step? I suppose that it is not a good approach, because drop growth and the changes of S are actually described by the same equation. Namely, when droplet grow they immediately decrease S. It is just the mass conservation law.

4. Most bulk schemes use saturation adjustment, which likely decrease the accuracy of those bulk schemes as compared to that used in RAMS. To what extent the values of corrected factors (eq. (4)) are suitable for other bulk schemes?

5. line 116. 1) Eqs (2) and (3) contain very strange notations: r is not the radius (typical notation), but mass mixing ratio. 2) from the notations it is not seen that rc in (2) is cloud water content (CWC), and in Eq. (3) rc is mass content of droplets belonging to the i-th bin in the bin scheme.
The utilization of the same notations to different quantities leads to confusion, and leads to the necessity of long explanations in the text. I would recommend to use bin indexes in case the bin scheme is discussed.

6. line 116. Most bulk schemes use saturation adjustment, which likely decrease the accuracy of those bulk schemes as compared to that used in RAMS. To what extent the results about the choice of the shape parameter (or corrections implemented in eq. 4) are suitable for other bulk schemes?

7. line 125. Table 1 present notations. The table does not present explanations. I suppose the expressions for condensational/evaporation growth should be presented clearer.

8. line 133. What is time step used in BULK in Eq. (2)?
The characteristic time scale of the change of S is drop relaxation time during which |S-1| falls trice. Time step should be smaller than the drop relaxation time. Otherwise utilization of the Eulerian integration scheme can lead to RH<100% in case of condensation. (This is the reason of the utilization of substeps in the bin-scheme).

9.line 158. It is not clear how do you use the approach to calculate S in the bulk scheme using the approach used in the bin scheme. Do you mean that you used analytic solution for S? How did you calculate coefficients in the equations supersaturations S and Si , which (i.e. coefficients) include size distributions? If you know supersaturation integral, why do you not use the bin-emulating procedure of recalculation of drop masses in each "bulk" bin?

10. line 172. The shape parameters may change with height because the shape of DSD changes. Sometimes the shape parameter should be changed together with other parameters of Gamma distribution.

11. line 186. There is no v in eq (3)

12. line 193. Correct typo.

13. Line 207 It is not clear how calibration can be performed when the bulk and the bin-schemes produce different droplet concentrations (because of different reasons including differences in aerosol concentrations).
If droplet concentrations are different, it means that the DSD shapes in BULK and BIN should be different just because the DSD shape depends on the droplet concentration.
It seems to me that it would be better to choose aerosol concentration in BULK in such a way to get similar droplet concentrations in BULK and BIN.

14. line 219. Supersaturation of 1% is quite large value. It is not clear why grid points with such and lower values were excluded from the analysis.

15. line 227. Fig. 2 is not clear. What is plotted in the figure? How were these figures obtained? Among many questions concerning this figure: why the condensation or evaporation rates are positive at any RH. Are these diagrams obtained by averaging over cloud volume? Over cloud life time?

16. line 238 In fig 3 "original", but not ORIG.

17. line 317. What are the values of the ratio f_nu , bin/f_nu_bulk? Are these bulk are time and spatial averaged?

18. If f_nu , bin/f_nu_bulk are calculated for each phase space bin, do you calculate a lookup tables to use in bulk simulations?
How would these values depend on the stage of cloud evolution and on cloud parameters (cloud top height). How would these values depend on aerosol concentration?
Can you present tables of these values? The application of formula (4) should be described clearer with examples of size distributions, the fields of CWC, fields of concentration, mean volume radius, etc.

19. line 338. Please provide DSD in bulk and DSD in bin before and after correction.

20. Please present comparison of fields of CWC (and concentrations) in bin and in bulk scheme before and after corrections. Only such comparison can say whether the correction introduced in (4) led to improvement of the bulk scheme.

21. line 433. I suppose that it is necessary to compare DSD in bin and bulk schemes. Otherwise it is impossible to understand what were the changes in the DSD in the bulk scheme as a result of correction expressed by eq. (4).

22. lines 440-444. The discussion is not clear. The changes in the shape (and amplitude) of DSD can be recalculated into the changes condensation/evaporation rates. So, these changes are closely related. Again, what were the changes in DSD predicted by bulk-scheme after correction expressed by (4)?

23. lines 462-472. The conclusions should be formulated better. First, which results of the authors justify that the Gamma distribution is a good assumption of the DSD? I did not find such justifications in the paper.
Second, immediately, the authors state that the exact knowledge of the shape is not necessary.
Third, immediately after these conclusions, the authors conclude that the shape parameter is responsible for agreement/disagreement with the bin -scheme results. All these statements seem contradict each other. The text should be shortened and rewritten clearer.

24. line 474. Despite the statement that the shape parameter is the main factor that allows to perform calibration, the procedure expressed by (4) does not correct the shape parameter, but just adjusts condensation/evaporation rates. What is the advantage of such approach vs the correction of the shape parameter itself.

It seems that this factor should depend on aerosol concentration

25. Line 485. The conclusions should be formulated clearer. What the authors propose to do with their bulk-scheme: to multiply the condensation/evaporation rates by some factor? Will this factor tabulated according to certain conditions, cloud stage evolution, etc.?