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Interactive comment on "LIMA (v1.0): a two-moment microphysical scheme driven by a multimodal population of cloud condensation and ice freezing nuclei" *by* B. Vié et al.

Anonymous Referee #2

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1 General Comments

This paper provides an overview of a new cloud and aerosol microphysics scheme called LIMA. One of the main advantages of the discussed scheme is its treatment of aerosol species, which is far more advanced than that which is commonly included in two-moment bulk models. I believe that this approach has the potential to make a significant impact on the cloud and aerosol modeling community and shed light on potential impacts of changes in aerosol loading on cloud microphysical properties. I also believe that the paper is appropriate for GMD. However, there are a few issues of concern that should be addressed before further proceeding with the publication

process.

2 Major Comments

1. **Model Physics and Equations**: There are some instances where the chosen terminology is either confusing/misleading or the chosen representation of a particular process may not be sufficient for tackling aerosol-cloud interaction problems. Regarding the former. The use of "the prognostic evolution of a three-dimensional (3-D) aerosol population" in the abstract is somewhat confusing. At first, I thought that the authors were extending the 2-D work published in previous papers, e.g., Bott (2000). The problem lies in the use of "three-dimensional". I think the authors should think carefully about the terminology that is used. For this example, perhaps omitting "three-dimensional" and using "the prognostic evolution of an aerosol population described by three characteristics" and then briefly list those characteristics would better describe the model.

   It is also not clear why the authors chose a factor of 1.2 times the rain water mixing ratio to demarcate the boundary between conditions without accretion and self-collection and those with these processes. Perhaps this just requires a reference or two. However, it may also be useful to ensure the reader that the predicted cloud properties are not largely affected by small changes in this parameter.

   I am also concerned about the lack of prognostic supersaturation in the model, especially given all of the effort that has clearly been taken to include a more physical representation of the ambient aerosol population. The authors clearly state that the model uses a saturation adjustment assumption. Under some circumstances, this is likely sufficient; however, in strongly forced environments, such an assumption may not be sufficient, again, especially if one is attempting to address aerosol-cloud interaction problems. Lebo et al. (2012) found that the inclusion of prognostic supersaturation in a two-moment bulk microphysics scheme was prudent for capturing aerosol effects in deep convective cloud systems, analogous to the squall line system examined in the current paper. Moreover, the authors showed numerical evidence for the presence of non-saturated conditions in convective updrafts, suggesting that using saturation adjustment may be inappropriate and physically unrealistic in such conditions. My guess is that the authors have good reasons for not including prognostic supersaturation; however, this is not clear in the paper. I think that at the very least, the authors should discuss the potential shortcomings related to excluding prognostic supersaturation and consider including this in a future version of the model. Moreover, given the lack of prognostic supersaturation, the wording in the conclusions should be softened a bit (e.g., removing "comprehensive" from "comprehensive treatment of warm phase processes").

One thing that is not address in the paper is how collisional processes affect the number of activated CCN and nucleated IFN. As collisions occur, the number of particles should decrease. Thus, the reactivation of evaporating drops, for example, may be incorrect if such a process is not accounted for.

Please also review Equation 7. Based on my calculations, the right-hand side is incorrect (albeit, it could be correct if there is an assumption that is being made but is not stated in the text). I get the following relationship:

$$LHS < \frac{\rho_a \sqrt{\psi_1 \omega}}{2\pi \rho_w G^{3/2} \psi_2} \left(\psi_1 \omega + \psi_3 \frac{dT}{dt} \right), \tag{1}$$

where LHS simply represents the left-hand side in the manuscript. The issue seems to be related to the combined term in the numerator of the right-hand side in the manuscript. Please review.

Similarly, is the power of 20 on the first equation on Page 7780 correct. The

power is 12 in the cited reference. Perhaps this is a unit conversion difference? If so, please make this clear in the paper.

Please check the units of K_1 and K_2; the units appear to be incorrect/inconsistent.

The approximation to the solution of Equation 8 was at first not clear to me. Perhaps being a bit more thorough would help the reader through the derivation, e.g., something as simple as "using the following assumption: for $x \ll 1$, $1 - e^x \approx x$".

On page 7784, the production of graupel at temperatures below -35°C is confusing. I think what you are saying is that frozen raindrops are added to the graupel category in the model at such temperatures. I think it is important to separate model assumptions from physics because, for example, graupel is formed via riming and is not necessarily a frozen raindrop.

2. **Test Cases**: I have a few concerns with the test cases chosen in this work.

   (a) Orographic Case: While I understand that the simulations are intended to be illustrative and for proof of concept, the model resolution seems a bit large (i.e., 5 km in the horizontal). Given that these are 2D simulations with a bulk scheme, I am not sure why a higher resolution was not chosen. I bring this up because I am concerned that some of the microphysical characteristics may be different if one used a higher resolution, e.g., 500 m. Moreover, where did the sounding come from for this case. The very low tropopause (i.e., near 400 mb) seems extremely low. The description of the simulation length is confusing; please explain more clearly how the results are presented in the figures and how long the simulations were. In general, I think it would be useful to explain the findings in the context of what we know about orographic clouds from observational and previous modeling efforts. For example, "indicate that black carbon is a more efficient nucleating agent than organics" makes it appears as though this is a result of the current work when in fact this has been established in prior works.

(b) Squall Line Case: As noted for the orographic case, it is unclear why the resolution is relatively coarse, i.e., exceeding 1 km. These simulations should be extremely short and thus it might be worth improving the grid spacing to better represent the processes that are important for the cases selected. Again, the results should be presented in the context of what we know about squall lines to better demonstrate the capabilities of the model.

3 Minor Comments

1. Page 7768, Line 20: The use of “The formation of hydrometeors is standard" is vague and unclear; please elaborate or exclude.
2. Page 7769, Line 3: The use of “captures" is misleading because the results are not compared to observed effects.
3. Page 7769, Lines 14-17: This sentence is very confusing. It is not clear how the modeling simulations are related to the “interplay". Moreover, the reference seems to be incorrect.
4. Page 7769, Line 25: Please consider using “plumes" instead of “puffs".
5. Page 7770, Lines 10-11: Please consider adding a reference or two here regarding the “trend".
6. Page 7770, Lines 16-17: Remove “grid size”.
7. Page 7771, Line 17: Remove apostrophe and ‘s’.

8. Page 7772, Lines 2-5: This sentence is very confusing. It is not clear what is meant by “resolution of the equation of the vertical motion".
9. Page 7773, Line 15: The terms “mode" and “modal" are used in several different contexts in the paper. Perhaps using “multi-categorical" would better convey the intended meaning and not confuse the reader?
10. Page 7781, Line 13: Remove the ‘s’ following the reference.
11. Page 7782, Line 7: Add ‘area’ before ‘mixing’.
12. In general, the use of lower-case ‘d’ and upper-case ‘D’ could be clarified or made consistent. It seems possible that the lower-case version is used for aerosol sizes, whereas the upper-case letter is used for hydrometeors. Is that correct?
13. In general, the figure captions could be bolstered with added details, especially regarding the times that are shown in the contour plots or the averaging that was done for other figures.
14. Lastly, there are many grammar issues (e.g., article usage, punctuation, verb usage, etc.) that should be addressed before resubmitting a revised manuscript.

References

Bott, A.: A flux method for the numerical solution of the stochastic collection equation: Extension to two-dimensional particle distributions, J. Atmos. Sci., 57, 284–294, 2000.

Lebo, Z. J., Morrison, H., and Seinfeld, J. H.: Are Simulated Aerosol-Induced Effects on Deep Convective Clouds Strongly Dependent on Saturation Adjustment?, Atmos. Chem. Phys., 12, 9941–9964, 2012.

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