Review of *LIMA (v2.0)*

Tarfour et al. present an extension of the "LIMA" microphysics scheme which adds prognostic number concentration to describe hail and ice hydrometeors. The additional process rates and assumptions are thoroughly documented, and the scheme is demonstrated with comparison to LIMA v1.0 in a deep convective cloud simulated in MesoNH. The source code is open source and documented for reproducing results. I have a few concerns related to figures, discussion, and the model description in this manuscript that warrant minor revisions, and otherwise believe it to be a strong contribution to the model development literature.

Comments:

1. One aspect of discussion is noticeably lacking in the introduction/conclusions: a comparison of the 2-moment prognostic approach in LIMA v2.0 with other existing 2-moment (and alternative) approaches. In particular I am thinking of the popular MG2008 and the P3 schemes. A few multimoment schemes are already mentioned in the introduction and citations, but without specific analogies to the new method presented. It would be ideal to understand specifically which prognostic variables and process rates are determined in a similar fashion to existing 2-moment schemes, and which ones have taken a more novel approach.
2. Several variables in section 3 are lacking a definition or reference. While many of these are free parameters that are listed in Table 1, others are not defined anywhere. For instance, what are…
    a. L182: the lambda_x range? Lambda is mentioned again in L505 without any reference to what this parameter corresponds to, or why it is important.
    b. Eq 7: rho_a (I assume air density?)
    c. Eq 13-15: free parameters a_y, b_y, c_y, d_y, rho_00, rho_dref, etc.
    d. L231 and Eq 17: T (air temperature or hydrometeor temperature?)
    e. Eq 19: g(D) ?
3. L290, the assumption of that "mean particle mass does not change", does not justified and should be clarified. For instance, the XMLT process leads to denser aggregates, which would imply that mean particle mass is increasing.
4. I would like to see a discussion of the complexity of v2.0 versus the single-moment v1.0. How many additional free parameters, prognostic variables, and process rates are required compared with the one-moment version? How does the time to run your 3D DCC simulation change with the addition of this complexity?
5. Several of the figures are very information dense, and could be improved to focus on specific quantities that are important to the new microphysics method. For instance:
    a. Figure 2 and 4: I suggest showing the hail precipitation/accumulation with colors (and either removing rain production, or using contour lines) rather than the patterns. Because LIMA v1 and v2 use the same prognostic variables for liquid microphysics, the differences in ice/hail hydrometeors is

the more interesting quantity, and is very difficult to see in the existing presentation.

b. I'm unclear why there is so much focus on updraft and downdraft magnitudes/locations in the results (ex. L351-355, Figure 3). My takeaway from figure 3 is that the simulations are dynamically similar (which one would expect since they use the same turbulence scheme and initialization), and thus differences in precipitation rates stem from microphysics, similar to the statement in L364-366. For this reason I believe you could eliminate the red/blue contours from figure 4 in order to make the results easier to read.
c. It is almost impossible to read the pie charts in figure 5, though I appreciate what the authors are trying to portray here. I think it would be more effective to display the full horizonal averages (hydrometeor concentration as a function of altitude) at the sacrifice of the isotherms and contours, which are challenging to read anyways. Then a more direct comparison between the altitude maxima and type of hydrometeors can be made for LIMA v1 and v2.
d. The legends/text in figure 8 should be made larger for readability.

6. In a few places, you mention that your results are "in line with conceptual schemes" (L348) or "in agreement with the observations" (L468), but it is not clear what these conceptual schemes state, or which observations are being compared. Please be more specific, especially since the conceptual scheme is mentioned again in L509.
7. In figure 9a indicates that the diagnostic relationship from LIMAv1 has an inverse relationship of snow mixing ratio and number concentration, contrary to the v2 results, which seems like a substantial difference in underlying assumptions. Can you address this discrepancy?
8. L468-475 provide an excellent summary of the key findings from the simulations!
9. The conclusions section could be improved to maintain focus on the findings of this study, and suggest a clear and specific path forward. In particular, the final two paragraphs focus on radar reflectivity and aerosol processing (which are not mentioned earlier in the paper) without making it clear which aspects of this future research are currently possible with LIMA v2.0, and which require further development. I suggest clearly stating that "future work is required" to enable comparison of radar reflectivity (and why it is a future metric), and that the prognostic number concentrations to LIMA 2.0 "enables future research" on aerosol impacts on hail and ice hydrometeors.

Other language/typos:

L11: "to produce" → "at producing"
L12: "to reduce" → "at reducing"
L151: what is a "releasable process" ? This wording doesn't make sense.
L182: "A new tables" → "New tables" or "A new table"
L302-305 is repeated twice (L206-L308)
L445: "dimensions of snow and graupel" ; what is the "dimension"? The mean size?
L447: "remain available" → "remains available"
L505: "observations Tarfour et al…" → "observations (Tarfour et al…)"