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Interactive comment on "Analyzing numerics of bulk microphysics schemes in Community models: warm rain processes" *by* I. Sednev and S. Menon

Anonymous Referee #2

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Major comments.

This paper addresses numerics in bulk microphysics schemes, mainly focusing on issues related to stability and positive definiteness. In general, numerical issues are critical for microphysics schemes, and have not received the level of attention by the parameterization development community that they sorely deserve, in my opinion. Therefore, I welcome studies such as this that bring these issues to the forefront, and thank the authors for their efforts in this area. That said, I do have several concerns about this particular study, primarily in terms of the context, interpretation, and presentation. These are described in more detail below. Overall I feel that major revisions are re-

quired before the paper can be considered for publication, though these revisions are significant enough that I certainly think it would be appropriate for the editor to reject the article and request later resubmission.

1. This authors make several significant claims for which they show no supporting evidence. In particular, in the abstract it is stated that "non-well-behaved" (the authors' words) schemes lead to "erroneous conclusions regarding the relative importance of different microphysical processes", problems in "precipitation and its spatial and temporal distribution", and they even claim that the numerics of these microphysics schemes "act as a hidden climate forcing agent". These claims are completely unsubstantiated by model results and are purely speculation. Such statements must be removed unless the authors test numerics in the context of regional and global model simulations, not just in their offline numerics tests. It is quite possible that the issues the authors' raise have limited impact on such simulations. In fact, this is suggested by the results of Gettelman et al. (2008, J. Climate) who showed limited sensitivity to sub time-stepping the microphysics, beyond 2 sub-steps (meaning 10-15 min time step). In other words, additional sub-steps did not produce any climatically-significant changes. Of course, sensitivity to time step is somewhat different than issues related to positive definiteness and stability that the authors' focus upon, in that it provides an even more stringent test because this type of sub-stepping also addresses issues related to time truncation errors (see also comment #3 below). Similar unsupported claims are made elsewhere in the paper, including p. 1406 and 1426.

2. The authors claim that using long time steps in current schemes can lead to instability and non-positive definite solutions, what they call "non-well-behaved" microphysics codes. However, this is exactly the purpose of the "mass conservation" technique, which leads to positive and stable solutions even at longer time steps. "Mass conservation" is analogous to the flux-adjusted approach, or the "fall through" approach that has been previously utilized to represent processes like sedimentation. The authors apparent solution to the problem - to simply sub-step the microphysics based on

the CFL-like stability condition (their Adaptive Substepping scheme), without relying on the mass conservation technique - is likely not practical in most models, and especially in GCMs, due to the computational expense (especially since it hasn't even been demonstrated that the mass conservation approach of current schemes actually leads to degradation of simulations anyway, as described in comment #1 above). I would welcome more elegant solutions to the problem besides "mass conservation" as long as there isn't a large increase in computational cost, perhaps using semi-implicit schemes as an example. I also disagree with the authors' contention regarding stability criteria for WRF (and CAM), i.e., that there is an additional time step limitation that needs to be imposed on simulations, since the "mass conservation" technique does provide stable and positive solutions.

3. In my opinion, the authors do not give enough emphasis on a related numerical issue - time truncation errors related to long time steps using the Explicit Euler method, due to nonlinearities of process rates. This is briefly mentioned on p. 1427, but the focus of this paper is almost exclusively on positive definiteness and stability. A related point is that the authors mention on p. 1426 that "a remarkable feature of these codes is that a minimum of two sub-steps are used even if stability and positiveness condition is occasionally satisfied. It makes this approach extremely computationally inefficient." However, a primary reason for substepping these codes (both use the MG08 microphysics scheme) is to address time truncation errors, not positive definiteness (which is addressed by the "mass conservation" approach). Such sub-stepping leads to tangible differences in climate simulations using 2 sub-steps, although not using additional sub-steps as described above in comment #1. This reviewer requests that the authors please clarify or remove these sentences from their paper, as it is misleading to the reader.

4.The paper is unnecessarily long and the presentation style is extremely repetitive. There are many points that are made several times in the paper. For example, the same point about "non-well-behaved" schemes is made on p. 1404, 1405, 1410, 1418,

1419, 1421, 1422, 1423, 1424, 1425, and 1426. This presentation style distracts from the main points of the paper. I also don't understand the necessity of separating 4.1, 4.2, and 4.3 into different sections. The analysis in all three of these sub-sections essentially shows the same thing - essentially, the SM stability criterion in Eqs. (27), (40), and (51) is the same. Overall, I think the length of the paper could be reduced to a total of 7-8 pages.

5. The abstract is too long, and its presentation style is more like an introduction than an abstract. The abstract should concisely summarize the main findings of the paper, which it currently does not do. There are also far too many acronyms used in the abstract and throughout the paper generally. Finally, the writing style of abstracts is typically not in first-person.

Additional comments.

1. p. 1404 "…stability condition for their explicit non-positive definite TIS was not defined." I disagree - with the "mass conservation" technique this scheme does guarantee positive solutions.

2. p. 1404, lines 27-28. Again, solutions are stable and positive with the "mass conservation" technique, see also comments #1 above.

3. p. 1406. As an illustration of the use of too many acronyms, why is "hidden climate forcing agent" given the acronym HCFA? This seems just odd.

4. p. 1406, line 16. This statement: "…share similar deficiencies of non-positive and unstable solutions in the autoconversion and accretion process if the microphysical time step used is greater than a few tenth of seconds." The authors' own results presented in Table 1 and Figs. 1-4 contradict this statement.

5. p. 1407. With the "mass conservation" technique used in current schemes, conditions expressed by Eqs. (4) and (5) are satisfied.

6. p. 1409. In my opinion it is misleading to say that positiveness criterion is never

checked. Although schemes generally do not sub-step the microphysics to ensure such positive definiteness, all schemes do check for positivity and adjust process rates as necessary to maintain positivity through the “mass conservation” technique.

7. p. 1410, first paragraph. Here is an example of where the authors place little emphasis on time truncation errors (see major comment #3 above). They state that a “remarkable feature of well-behaved EEBMPC is that it assures a correct solution for governing differential equations.” However, “correctness” of the solution is never defined. Even if the scheme is “well-behaved”, as the authors define it, it could still have significant truncation error and lead to inaccurate solution due to nonlinearity of the process rates.

8. p. 1410, line 10. “. . ..that both rely on so called ‘mass conservatoin’ techn ique in an attempt to avoid negativeness of hydrometeors’ mixing ratios and EE TIS positive definite.” These schemes don’t “attempt” these points, they accomplish them through mass conservation technique.

9. p. 1412. Sentence starting with “These vertical profiles provide a thoughtful way. . .” is not clear.

10. p. 1413, lines 5-6. It is stated that WRF simulations with a time step larger than in the inequality in (16) leads to unstable and non-positive definite numerical solutions. Again, this is not true because of application of the “mass conservation” technique.

11. p. 1413, Eqs. (17)-(18). Variables Qc0 and Qr0 are not defined (I’m assuming these are quantities at time t = 0, but this is never stated).

12. p. 1414. It is confusing to say that Eq. (27) doesn’t depend on the specific formulations for autoconversion and accretion growth rates, because these rates appear in Cu0 and Ca0. It is clear that the condition expressed in (27) will depend on specific formulations for PAUTO and PACCR.

13. p. 1419, lines 5-6. The Morrison et al. and Morrison and Gettelman schemes

implemented in WRF and CAM are different schemes, not the same scheme as implied here.

14. p. 1420. It is stated that “reduced artificial autoconversion AAUTO and accretion AACCR rates are used” through the mass conservation technique. This is only done as needed to maintain stability and positivity, not in all time steps as the reader might be led to believe by this statement as well as the following one at the top of p. 1421.

15. p. 1421. This statement is confusing and requires clarification: “It is worth noting that output arrays of non-well-behaved EEBMPC passed to a host model contain artificial numbers that are chaotic at different times, altitudes, and geographical locations and should not be used for post-processing analysis. . .” What is meant by “chaotic” here? This is a strong statement and the authors need to be very specific what they mean here.

16. p. 1421. The analogy of numerical solution of microphysics with 1-D advection equation with positive constant velocity Cadv is a poor one. For microphysics, Cadv is absolutely not constant, since the rate of loss (or gain) depends on the quantity itself (Qc or Qr). To use the analogy of using constant Cadv for microphysics is not reasonable. On p. 1422, line 4, the authors again say that solution using mass conservation is inconsistent with the definition of Cadv as a constant, but again, Cadv is not constant for microphysics. The authors are therefore invoking a straw-man argument.

17. p. 1422. It is stated that the “mass conservation” technique with adjusted rates (if needed) contradicts the linearization used to the derive the finite difference equations, but this linearization is of course only an approximation anyway.

18. p. 1422, line 13. The authors state that a “All non-well-behaved” EEBMPC calculates growth rates due to microphysics incorrectly”, but it is not clear precisely what is meant by “incorrect” here.

19. p. 1422, second paragraph. If all cloud water is depleted within a time step using

the mass conservation approach, what is the practical difference if the microphysics time step is set such that SM stability criterion is exactly satisfied? In both instances, there will be no cloud water at the end of the time step.

20. p. 1423, lines 1-3. It is stated that the MG08 microphysics scheme implemented in the CAM and AM3 GCMs uses two substeps to avoid numerical instability, but more specifically it addresses time truncation errors using the Explicit Euler method (see major comment #3 above).

21. p. 1424, line 1-2. The authors state that a "well-behaved" EEBMPC provides assurance of correctness of the numerical solution, but "correctness" is not defined. There can still be large time truncation errors leading to inaccurate (if stable and positive definite) solutions, for example.

22. p. 1425, lines 8-11. It is stated that the mass conservation technique does not eliminate numerical instability that might arise using long time steps, but this is not demonstrated.

23. p. 1425, line 16. The authors state that warm rain growth rates (from autoconversion and accretion) are "known constants" calculated at the beginning of each time step and cannot be changed. These rates are constant during a time step only because of linearization, which is only a (rough) approximation of the real solution. The rates themselves are not constants, but depend strongly on the existing cloud and rain water amounts.

Technical comments.

1. p. 1404, line 25. "hundredths" should be "hundreds"

2. p. 1411, line 19. "two or three hundreds" should be "two or three hundred"

3. p. 1424, line 14. "hundredths" and "thousandths" should be "hundreds" and "thousands"

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