Geoscientific
Model Development
Discussions

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

I. Sednev and S. Menon

isednev@lbl.gov

Received and published: 27 September 2011

We would like to thank the reviewer for reviewing our paper entitled "Analyzing numerics of bulk microphysics schemes in Community models: Warm rain processes".

We agree with the reviewer's opinion that numerics used in BLK schemes is of crucial importance despite the fact that it was out of the focus of the modeling community for many years. We would like to add that a comprehensive theoretical analysis of numerics used in BLK schemes implemented in WRF and CAM has never been done.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Reviewer's comments are in *italic*

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 required 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.

We agree with the reviewer that the importance of numerics used in BLK schemes was underestimated by the parameterization development community. We also agree that analysis of such issues as stability and positive definiteness of numerical solutions in an explicit Eulerian time integration framework are important points in our paper. We have noted the concerns of the reviewer and our response and revisions to the paper should hopefully clarify our paper and the discussions presented. Our detailed response to all queries are below.

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.

We understand the concern the reviewer has expressed in that we need to demonstrate that the errors in microphysical schemes used in models highlighted in our work do indeed have an impact on climate. The reviewer suggests our results are speculative unless we demonstrate that the results hold true in regional and global simulations and not just in "offline numerical tests" (as he defined our theoretical analysis). In the next statement the reviewer does indicate that the issues we raise may have limited impacts and that the work of Gettleman et al. show that differences with additional sub-steps are not climatically significant. However, we differ with the reviewer on these points. Our paper analyzes the numerics used in bulk microphysics codes in community models and we show how stability and non-positive situations encountered in

these codes due to longer time steps used can lead to errors. We also show analytically the time steps that maintain the stability and the positive definiteness of the time integration scheme for warm cloud microphysical processes. We recognize that sometimes these time steps might be too small to implement in large-scale models and also recognize that their significance needs to be demonstrated before recommending its implementation in large-scale models. This is ongoing work and is the subject of our second paper. We have now included this discussion in our revised paper. Our current preliminary research indicates that the implementation of this scheme in an idealized WRF simulation affects the spatial and temporal patterns of precipitation (for example, maximal accumulated precipitation amount can be higher by much as 30 % - 80 %). In fact preliminary results using the latest version of CAM indicates that sub stepping to 4 or 6 time steps instead of 2 does affect precipitation patterns regionally (not as much globally) and this increase in sub steps results in a 1 $W\,m^{-2}$ change in TOA radiation. Results from Gettleman et al. or for that matter Morrision and Gettleman show zonal means or vertical profiles when examining sensitivity to sub stepping. Our results indicate spatial and temporal patterns of precipitation are also affected. We will include these additional statements in our revised paper to clarify why the results we show may be important for large-scale models as well.

On the issue of sensitivity of time step and time truncation, the reviewer refers us to comment #3 and our response is indicated under comment #3. With regard to unsubstantiated claims on pages 1406 and 1426, we are not sure what the reviewer refers to, but in case these refer to our assertion that numerical errors could as well be a hidden climate forcing agent, we have modified our statements on both pages to suggest that the errors could be large enough to have an impact on radiation (e.g. the 1 $W\,m^{-2}$ TOA radiation difference with smaller sub steps is of similar magnitude as the aerosol indirect effect) similar to the magnitude obtained from aerosol climate effects. We have noted the reviewer's concern and have removed the phrase "hidden climate forcing agent".

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 is not 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.

On page 1425 we do state that microphysics schemes in WRF that use mass conservation belong to the conditionally well behaved EEBMPC class when used for cloud resolving or large-eddy simulations. Our assertion is that it gets violated when used for larger scales with longer time steps. That is the basis of the paper in which we provide a numerical framework with solutions that may be applied when these codes are used for large-scale simulations. We agree that the computational expense can be prohibitive. However, with increase in computational resources and the advances in areas such as multi-scale modeling framework where CRMs are embedded in grids of climate models to explicitly resolve cloud processes, the need to rely on "mass-adjustment" schemes to avoid negative condensate values no longer is necessary. Rather, it is important

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

to develop a framework that is physically and numerically consistent and that can be applied in any model regardless of scale. That is the purpose of this work.

Based on the reviewer's statement "... the purpose of the mass conservation technique, which leads to positive and stable solutions even at longer time steps" we recognize that the reviewer missed the main point of our paper, that is the derivation of a general analytical condition (SM-criterion) that remains valid regardless of parameterizations used for autoconversion and accretion and which determines the existence of an unique positive-definite stable numerical solution in an explicit Eulerian time integration framework used in BLK schemes under consideration. A few major comments and many additional comments are based on the assumption that the so called "mass conservation" technique is a legitimate mathematical approach that can be applied to avoid negativeness of hydrometeors' (cloud water in our case) mixing ratio. Our analysis in subsections 4.1-4.3 shows the NONEXISTENCE of an unique positive-definite stable numerical solution in an explicit Eulerian time integration framework for differential equations that govern warm rain microphysical processes for microphysical environmental conditions for which $N_{sm} > 1$. This conclusion implies that any additional assumptions not included in the strict mathematical definition of the problem are not valid. One of these additional assumptions is the extrapolation of the existence of a positive-definite explicit Eulerian numerical solution to a time interval that is greater than that given by the general SM-criterion. The latter assumption is a quintessence of the so called "mass conservation" technique that assumes that "adjusted" growth rates are applicable at a time interval where a numerical solution does not exist. An understanding of the fact that the numerical solution does not exist on an arbitrary chosen time interval is sufficient to reject the utilization of the "mass conservation" technique, and any additional proof for its rejection is not needed.

We respect the reviewer's disagreement 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". Coupling between a host model numerics and a

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

BLK scheme numerics is a challenging problem. More generally, for various reasons, a unified time integration framework for different model components has not yet been developed. It has not yet been demonstrated how the utilization of different time integration frameworks in different model components influence general model behavior. We agree with the reviewer's point of view that numerical schemes different from explicit Eulerian time integration should probably be used in BLK schemes. It became evident for us that the development of parameterizations for different microphysical processes should be separated from its numerical implementation. That is why we are working on the development of a prototype of a so called Open Flexible Microphysics Interface (OpenFMI). OpenFMI consists of a suite of different time integration schemes whose utilization is application dependent and contains a process-oriented source code repository with libraries that include functions for calculations of hydrometeors growth rates due to different microphysical process routinely used in different BLK schemes. Advantages of OpenFMI are that 1) it avoids the necessity to account for special numerics issues such as stability and positive definiteness (among others), 2) it permits the researcher to easily incorporate their findings into a unified numerical framework that provides them with unified function templates that should be filled in, and 3) it provides computational infrastructure flexibility in choosing description of different microphysical processes, i.e. the ability to build "bulk microphysics schemes on the fly" by using re-usable functions from its repository and libraries developed by others. Concerns and additional statements are now included in the revised paper.

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 micro-physics 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.

"Time truncation errors related to long time steps using the Explicit Euler method" are of minor importance as compared to the existence and uniqueness of a numerical solution related to this long timestep because "these errors" have no physical basis in an unstable and non-positive definite scheme. Although "time truncation errors" are discussed in the MG08 paper there is no discussion on the magnitude of these errors. It is thus difficult to understand a conclusion in the MG08 paper that substepping the precipitation microphysical processes in time was necessary to minimize "time-truncation errors". In our revised paper we now include a statement that a numerical scheme preciseness and time truncation errors would also be a numerical issue to consider if there is a proof that this numerical scheme is stable and positive definite.

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.

We use the phrase "non-well-behaved EEBMPC" frequently but this phrase is used in a different context each time. We have revised the paper to make it more concise where possible and less repetitive. However, subsections 4.1-4.3 cannot be "synthesized" or omitted because each section has a different meaning and is important by itself. In subsections 4.1 and 4.2 we deal with different equations that are differential-difference equations and finite-difference equations, respectively. We also analyze the existence of an analytical solution for linearized equations and a numerical solution in an explicit Eulerian time integration framework, provide a time interval for which analytical and numerical solutions exist, and derive the necessary stability and positive-definiteness conditions for an explicit Eulerian time integration scheme.

The first step in a comprehensive analysis of a numerical scheme is an attempt to solve differetial-differences equations analytically. Recall that RHSs are "frozen" according to the linearization applied. In subsection 4.1 it is shown that a simple analytical solution that satisfies mass conservation equation (11) and given by (23)-(24) exists for any time interval for which the linearization of RHSs (assumption that they are constants) remains valid (for example, due to physics consideration) regardless of the parameterization of autoconversion and accretion process. However, according to the definition of the general problem (given in Section 2), this solution has to be positive as formulated by constrains (4)-(5). For an analytical solution (23)-(24) these constrains are given by (25)-(26) and permit the calculation of a maximal time interval for which a positive analytical solution for the linearized problem exists. We would like to highlight that the strict mathematical definition of the problem given in Section 2 is of crucial importance because it bans the utilization of any additional assumptions or constrains. The last sentence in Section 2 is very important because it states that whereas initial conditions (IC) and RHSs (given by particular parameterizations that are different in different BLK schemes) are known, the time interval for which this solution exists is dependent on both ICs and RHSs and has to be calculated. It should be noted that this time interval can be calculated even before the microphysical equations are numerically solved in

an explicit Eulerian time integration framework.

Summarizing, in subsection 4.1 a) a mass-conserving positive-definite analytical solution for linearized differential-difference equations that govern processes of warm rain formation in BLK scheme is provided; b) it is shown that the time interval for which an analytical solution exists is determined by the SM-criterion for the linearized differential-difference equations (27). At this point it should be clear that any assumption regarding the existence of an analytical solution for a time greater than that given by (27) has no mathematical sense. An analytical solution for linearized differential-difference equations permanently exists for any "t" on time interval $0 \leq t \leq \tau_{max}$ only.

The second step in our comprehensive analysis of the numerical scheme is an attempt to solve the finite-differences equations numerically. Once again, RHSs are "frozen" according to the basic requirement of an explicit Eulerian time integration framework. In subsection 4.2 it is shown that a finite-difference explicit numerical solution given by (36)-(37) satisfies the finite-difference analog of the mass conservation equation (11), and thus is mass-conserving. However, this solution is not positive-definite. In addition to the finite-difference equations, constrains to ensure positiveness of the finite-difference solution (36)-(37) are given by (38)-(39). These constrains determine the necessary condition (40) for the explicit Eulerian finite-difference scheme (34)-(35) to be positive definite regardless of the parameterization formulae used for autoconversion and accretion growth rates. An observation that the solution (23)-(24) for differential-difference equations and the solution (36)-(37) for finite-difference equations coincide is extremely important, and its mathematical meaning is that the finite-difference scheme is stable for fixed timesteps that do not exceed the maximal timestep given by the SM-criterion for the finite-difference equations (40). Trying to keep our text as simple as possible we do not provide additional "excessive" strict mathematical definitions of stability that can be found in text books on numerical methods used to solve differential equations. At this point it should be clear that any attempt to solve the finite-difference equations (34)-(35) using a timestep that is greater than that given

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

by (40) has both no mathematical and physical sense because this situation is not governed by these equations. For a different time integration framework, the positive-definiteness condition and stability condition might differ. Stability is a very important issue that makes finite-difference equations different from differential-difference equations for which the stability problem is not relevant. Analysis of stability is of crucial importance for any finite-difference scheme and should be done before its implementation in a numerical model.

The third step in the comprehensive analysis of a numerical scheme is stability analysis. Even though in subsection 4.2 it was shown that an explicit Eulerian scheme is stable and positive definite (when the SM-criterion is satisfied) in subsection 4.3 we provide a different way of analyzing the stability of finite-difference equations. This method can also be used for the different time integration frameworks that are more complicated than an explicit Eulerian framework as well as for more complicated governing differential equations (when "other process rates" are under consideration). Analysis of more complicated cases is outside the scope of our paper.

In our revised paper we try to differentiate the relevance of each subsection more clearly for those who are unexperienced with numerics.

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.

We have revised the abstract as suggested by the reviewer.

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.

We state that not only stability of "prognostic water plus diagnostic rain" scheme implemented by MG08 in CAM has never been defined but also a comprehensive stability analysis of these type of schemes has never been done (to the best of our knowledge). Even if non-mathematically based "mass conservation" technique "does guarantee positive solutions" how does it ensures stability? Sensitivity tests in MG08 for idealized cases can not be thought of as a substitution to a theoretical analysis.

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

An explicit Eulerian numerical solution is not stable and positive definite for a time interval that is longer than that given by the SM-criterion. A positive definite stable numerical solution simply does not exist on this time interval.

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.

Agreed. We have removed the acronym for HFCA and references to this term.

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.

Our figures show a broad range of cloud and rain water mixing ratios that may exist in the real atmosphere for two values of cloud droplet concentrations that are different

by an order of magnitude. Because it is conventionally thought that the WRF model can be applied for a broad range of spatial scales and different cloud types our general statement (without identifying a particular scheme, horizontal scale of simulation, and cloud type) remains valid; however, the word "might" is obviously missing. The phrase should read ". . .might 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 tens of seconds."

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

This statement is not correct because the quintessence of the "mass conservation" technique is assumption that "adjusted" growth rates are applicable at a time interval $t > \tau_{max}$ where a numerical solution does not exist; whereas Eqs. (4) and (5) are only satisfied for a time interval $0 \leq t \leq \tau_{max}$. Please also see our response to major comment #4.

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.

Growth rates cannot be "adjusted" because their values are known constants at the beginning of each timestep and can not be changed in an explicit Eulerian time integration framework. Regarding maintenance of "positivity through the mass conservation technique" please read our response to major comment #2.

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.

In the context of our paper correct solutions mean unique positive-definite stable solutions as it follows from the analysis given in section 4. Our analysis provides the maximal time interval (given by the SM-criterion) for which this unique positive-definite stable solution exists. The preciseness of this solution is application dependent. Additional sentence reflecting the reviewer's statement will be included in the revised paper.

8. p. 1410, line 10. ". . ..that both rely on so called 'mass conservatoin' technique 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.

Our point is that "They accomplish them through mass conservation technique" in a mathematically incorrect way as explained in our response to major comment #2.

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

We have revised the statement by changing "thoughtful" to "useful".

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.

This comment is misleading to us. In our paper we stated that "...for regional or large scale WRF simulations with a time step chosen according to inequality (16) violation of the SM-criterion at different times, altitudes, and spatial locations leads to unstable and non-positive definite numerical solution for the governing warm rain differential

equations". As opposed to the reviewer's comment, we state that if time step is chosen according to (16) as provided in the WRF User Guide, but the SM-criterion is more restrictive, the maximal timestep should be less than that given by the condition N_{sm} = 1. If the timestep chosen for the WRF simulation is "larger than in the inequality in (16)" the numerical solution is unstable for dynamical reasons regardless of the microphysics. Inequality (16) accounts for the limitation on the timestep due to the dynamics, whereas the SM-criterion accounts for the limitation on the timestep due to the microphysics. In general, a more restrictive timestep should be used for WRF simulations.

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).

Yes. These are now defined in the revised paper.

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.

Expression (27) does not depend on the specific formulation of PAUTO and PACCR. For linearized equations it is a general expression that remains valid regardless of the specific formulations for autoconversion, PAUTO, and accretion, PACCR, growth rates. However, it is obvious that the numerical values of the maximal time interval calculated according to (27) are different for different schemes.

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.

We are aware of this. We have never stated that both schemes are identical. In our

paper we discuss the numerics. From this perspective the MORRISON scheme implemented in WRF that utilizes prognostic equations for precipitating hydrometeors is different from the CAM scheme. In the Discussion section it is clearly stated on p. 1426 that "...both CAM (Gettelman et al., 2008) and GFDL AM3 GCM (Salzmann et al., 2010) utilize diagnostic equations for precipitating hydrometeors, but the numerical treatment of cloud water remains similar to that used in an EEBMPC with prognostic equations".

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.

We have never claimed that violation of SM-criterion occurs at every time step. On page 1420 (lines 11-15) it is clearly stated that "Although the solution (58)-(59) conserves mass, it is not positive definite. Whereas q_r^{n+1} is always positive, q_c^{n+1} sometimes might be negative because the positiveness condition given by SM-criterion..." is not satisfied. Thus, "reduced artificial autoconversion, AAUTO, and accretion, AACCR, rates are used (through the mass conservation technique)" to avoid negativeness of cloud water mixing ratio if the SM-criterion is not respected.

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.

For example, growth rates are calculated incorrectly as explained later in our answer to additional comment #18. The amount of precipitation on the ground is another exam-

ple. We agree that clarification is needed in our statement and have clarified accordingly. We removed the word "chaotic" as well.

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.

This reviewer misinterprets our example. Our example shows that for a stable numerical solution for the advection equation with a given constant advection velocity on equi-distant grid, the CFL-criterion should be satisfied. This prototype equation is routinely used in the analysis of numerical schemes. The CFL-criterion depends on two constants (velocity and grid size) and one variable (timestep). Only the timestep can be changed because the other two parameters are constants by definition. Many years ago when the CFL-criterion was not known, numerical schemes used to solve advection equations sometimes utilized time steps that was greater than that given by this criterion. However, when the CFL-criterion was established it was recognized that any attempt to solve advection equations utilizing time step that is greater that that given by the CFL-criterion results in an unstable solution in an explicit Eulerian time integration framework. Nowadays, it makes no sense to solve advection equations using an explicit Eulerian scheme with a time step for which the CFL-criterion is disrespected. Up to know there has not been a link to microphysics. In the case of the utilization of "mass conservation" in BLK schemes, growth rates (similar to advection velocity in the advection equation) and cloud water mixing ratio are known constants in an explicit Eulerian time integration framework in which the SM-criterion has to be respected. To satisfy this criterion only the time step can be changed because growth rates are constants and cannot be "adjusted".

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.

Linearization means "unchanged". Growth rates are constants by definition and cannot be "adjusted" in an explicit Eulerian time integration framework that is of course only an approximation by itself. Once again, the utilization of this framework in BLK schemes under consideration is not our decision. In our paper we mainly analyze the numerics of these schemes.

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

The "mass conservation" technique as implemented disrespects the original intent of the parameterization developed by utilizing "artificial" numbers instead of the correct formulae. For example, in the case of the MORRISON scheme analyzed in section 4.4 these numbers are given by our expressions (61) and (62) instead of the original formulae (56) and (57), respectively, provided by KK2000. The "adjusted" growth rates used in MORRISON depend on a timestep τ not found in the original KK200 formulae. This is what we meant by incorrect (more detailed explanation are provided in our response to the specific comment #16 of reviewer #3). We have clarified our explanation in the revised manuscript.

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.

The reviewer "correctly" states that in the case of "mass conservation" the SM-number is ALWAYS equal to one. It would be absolutely correct if the reviewer means "vir-

tual" SM-number (regarding "virtual" microphysics reality and virtual N_{sm} please see our response to the specific comment #16 of reviewer #3). A practical difference, for example, is that sedimentation equations should not be solved for a time interval for which a solution does not exist.

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).

Our statement is based on the abstract of the MG08 paper: "It is found that, in general, two substeps are required for numerical stability and reasonably small time truncation errors using a time step of 20 min" and later in the paper where it is stated "Thus, we explore substepping in time to address numerical instabilities and time truncation errors". Moreover, it is thought that Fig. 1 in the MG08 paper demonstrates how numerical instability can be eliminated by increasing the number of substeps from one to two. At the same time phrase "time truncation error" is found in abstract, pp. 3651 (twice), 3652, and discussion whereas definition of "time truncation error" for "prognostic cloud water plus diagnostic rain water algorithm" has never been provided (for example, for diagnostic equations there is no "time" at all and a sense of "time truncation error" is not obvious). Additionally, in the MG08 paper "time truncation error" has never been quantified and even discussed. Because the reviewer uses phrase "time truncation errors" repetitively and claims that the MG08 paper "addresses time truncation errors using the Explicit Euler method" we will include corresponding discussion in our revised paper as it was already mentioned in our response to major comment #3.

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.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

In the context of our paper, correct numerical solutions mean unique positive-definite stable numerical solutions. In our paper we do not discuss preciseness of this unique solution. We focus on the derivation of the necessary condition (SM-criterion) for the existence of an unique positive-definite stable numerical solution in an explicit Eulerian framework that is used in BLK schemes under consideration (please also see our response to major comment #3).

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.

This statement is the direct conclusion of our analysis. Artificial "adjustment" of the original growth rates or decrease of the timestep are only alternatives in the explicit Eulerian time integration framework. Reviewer's comment is not fair because our statement is taken out of context. Please, read previous and following sentences in the paper as well as our response to major comment #2, where consequences of the utilization of "mass conservation" are explained in more detail.

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.

Linearization in an explicit Eulerian time integration framework means "unchanged during a given timestep". This time integration framework implies that RHSs of finite-difference equations (that are linear or non-linear with respect to unknown variables) are known constants. Growth rates are not functions of their arguments during a given timestep, because growth rates are known constants that can depend linearly or non-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

linearly on known cloud and rain water mixing ratios (among others) supplied by a host model. The decision to use an explicit Eulerian framework was made by those who implemented BLK schemes under consideration. We only analyze what has been done, and a focus of our paper is not if an explicit Eulerian time integration framework is a rough or excellent approximation.

<center>Technical comments.</center>

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"

We definitely will correct these words.

References

Gettelman, A., H. Morrison, and S. Ghan: A new two-moment bulk stratiform cloud microphysics scheme in the Community Atmosphere Model, version 3 (CAM3). Part II: Single-column and global results. J. Climate, 21, 3642-3659, 2008.

Khairoutdinov, M., and Y. Kogan: A new cloud physics parameterization in a large-eddy simulation model of marine stratocumulus. Mon. Wea. Rev., 128, 229-243, 2000.

Morrison, H., J. A. Curry, and V. I. Khvorostyanov: A new double moment microphysics scheme for application in cloud and climate models. Part I: Description. J. Atmos. Sci., 62, 1665-1677, 2005.

Morrison, H. and A. Gettelman: A new two-moment bulk stratiform cloud microphysics scheme in the Community Atmosphere Model, version 3 (CAM3). Part I: Description and numerical tests. J. Climate, 21, 3642-3659, 2008.

Salzmann M., Y. Ming, J.-C. Golaz, P. A. Ginoux, H. Morrison, A. Gettelman, M. Kramer, and L. J. Donner: Two-moment bulk stratiform cloud microphysics in the GFDL AM3 GCM: description, evaluation, and sensitivity tests. Atmos. Chem. Phys., 10, 8037-8064, 2010

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper