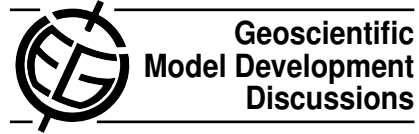


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Interactive
Comment

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

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We would like to thank P. Caldwell for commenting on our paper entitled "Analyzing numerics of bulk microphysics schemes in Community models: Warm rain processes".

Based on a few P. Caldwell's statements like "... All of these schemes *are* positive definite because of their "mass conserving" aspect..." or "I think mass-conserving schemes will actually be very stable because they just zero out any unreasonable liquid. In fact, they probably get more stable at longer timesteps" we recognize that he relies on the assumption that the so called "mass conservation" technique is a legitimate mathematical approach that can be applied to avoid negativeness of cloud water mixing ratio. At the same time these statements indicate that he missed one of the main points of our paper. This point is the derivation of a general analytical condition ($N_{sm} \leq 1$) that determines the existence of an unique positive-definite stable numerical solution in an explicit Eulerian time integration framework and remains valid regardless of parameterizations for warm rain processes used in BLK schemes under consideration.

Reviewer's comments are in *italic*

1. The authors' conclusion seems to imply that we should use CRM-length timesteps for climate integrations. This is clearly not computationally feasible, but the authors provide no alternatives. Other possibilities such as integrating the sum of the dominant cloud water sources and sinks rather than independent integration of the source terms and sink terms or using an integrating factor approach with exponentially-decaying sink terms seem more reasonable but are not even mentioned.

Cloud physics considerations imply that all formulae for growth rates due to different microphysical processes would be valid only if small time steps are used for time integration of microphysical governing equations for hydrometeors' mixing ratio (single moment BLK scheme) and mixing ratio and concentration (double moment BLK scheme) in an explicit Eulerian time integration framework.

The authors' conclusion implies that, from numerics point of view in the context of our paper, there is no a big difference between bin and bulk microphysics schemes. As opposed to a bin scheme that solves governing integro-differential equations for size distribution functions for different hydrometeors directly (e.g., Sednev et al., 2009), a bulk scheme uses over-simplified approach based on a priori known analytical representation of size distribution functions and solves governing differential equation for one or more moments of these functions. Both types of these microphysics schemes are routinely used in cloud-resolving models. However, mechanistic extrapolation of their applicability to a large scale arises mathematical problems as our analysis shows. Additionally, many parameterizations for different microphysical processes use formulae for local growth rates that cannot be applied for simulations on a larger scale or environmental conditions quite different from those for which these formulations were developed. For example, using a bin-resolved microphysics scheme with 3D LES model Khairoutdinov and Kogan (Khairoutdinov and Kogan, 2000) (KK2000) developed a warm bulk microphysics scheme for particular cloud type. Despite the fact that KK2000 clearly stated that " ... it may not be valid to extrapolate its use to other cloud types and conditions", KK2000 formulae for autoconversion and accretion are used in different BLK schemes. For instance, the MORRISON scheme implemented in WRF uses KK2000 formulae without providing any indication of its applicability range. Obviously, that this scheme can not be used for a simulation of development stage of convective clouds (Morrison et al., 2009) when vertical velocities reach maximal values of 25 m s^{-1} during the first two hours. However, KK2000 formulation can be applied for conditions with stratiform precipitation observed during last few hours of their simulations with a time step equal to five seconds. Another example is implementation of KK2000 formulae in convective parameterization for use in GCM (Song and Zhang, 2011) whereas these formulae are not applicable for convective clouds. These examples shows that not only mathematical (as our analysis show) but also cloud physics problems arises when BLK schemes are utilized in atmospheric models on large scale.

According to the title of our paper we analyze numerics of warm rain processes in FOR-

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TRAN source codes for BLK schemes that are publicly available. Analysis of numerics of BLK schemes in mixed-phase clouds and differences between warm and mixed-phase cases is discussed in separate paper that is in preparation. Another possibilities mentioned by P. Caldwell might be discussed if available.

2. The paper seems unnecessarily complex. For example, the SM criterion is just saying that the timestep should be small enough that microphysics doesn't deplete more than the available cloud water in a single step, but this fact is hidden behind unnecessary algebra and never really stated. Also, sections 4.1 and 4.2 are redundant since forward-Euler integration is exactly equal to analytic integration once autoconversion and accretion rates are linearized.

P. Caldwell underestimates and misinterprets significance and importance of the SM-criterion that constitutes non-existence of a unique stable positive-definite numerical solutions for the problem under consideration. The statement regarding "complexity" of our paper is confusing. In contrary to this statement we are sure that presentation in sections 4.1 and 4.2 is oversimplified. Additionally, sections 4.1 and 4.2 are not redundant since in these subsections we consider different types of equations that are differential-difference equations and finite-difference equations, respectively. Moreover, the fact that an analytical solution is exactly equal to a numerical solution in an explicit Eulerian time integration framework is very often used as a proof of stability of a finite-difference scheme that can be found in text books on numerical methods used to solve differential equations. (Please see our response to Reviewer #2, major comment #4).

3. All analysis in section 4.1 - 4.3 is based on the assumption that autoconversion and accretion is linear in q_c and q_r . This is typically not true for a microphysics scheme, so claims that the stability constraints hold "regardless of parameterization" (p1418, L15) are not true.

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P. Caldwell's statement that "All analysis in section 4.1 - 4.3 is based on the assumption that autoconversion and accretion is linear in q_c and q_r " is completely false. Words "linear" and "linearization" have very different meaning. Both autoconversion and accretion rates given by formulae (30)-(33) are nonlinear in " q_c^n " and " q_r^n ". Brief observation of (32)-(33) shows that both PAUTO and PACCR are any functions that depend (linearly or non-linearly) on q_c^n and q_c^n and q_r^n , respectively; however, their values are known at the beginning of each microphysical time step. These values cannot be changed in an explicit Eulerian time integration framework that implies that right hand sides of the differential equations (which are linear or non-linear with respect to unknown variables) are known constants. Thus, "linearization" in an explicit Eulerian time integration framework means "unchanged during timestep". In contrary to P. Caldwell's statement, our assertion that the SM-criterion provides the necessary condition for the explicit Eulerian finite-difference scheme given by Eqs. (34)-(35) to be stable and positive definite regardless of parameterization for autoconversion and accretion growth rates is completely correct.

*4. p. 1421: If you compute τ_{max} for mass-conservation-adjusted autoconversion and accretion, you will find that $\tau_{max} = \tau...$ in other words, no matter what timestep you choose, you will exactly remove all the water in that timestep. This means that the schemes implemented in Morrison and elsewhere *do* actually satisfy the SM criterion and as a result they can't be criticized from a numerical perspective.*

Caldwell's statement that "This means that the schemes implemented in Morrison and elsewhere *do* actually satisfy the SM criterion and as a result they cannot be criticized from a numerical perspective" is completely misleading. However, if somebody computes τ_{max} by using "virtual" cloud water and rain water mixing ratios in original KK2000 formulae instead of their "real" values supplied by a host model one would find that an algorithm implemented in the MORRISON scheme does actually rely on a "virtual " SM-criterion but not the "real" SM-criterion (regarding a "virtual microphysics reality" introduced by utilization of the "mass conservation" technique see our response

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to Reviewer 3, comment #16). However, we prefer not to deal with the "virtual" micro-physical environment introduced by utilization of the "mass conservation". Before our work there was only one method to ensure non-negativeness of cloud water mixing ratio in an explicit Eulerian time integration framework by using the "mass conservation" technique even if all mathematical and physical consequences of its utilization were not known. However, nowadays, there is an additional opportunity that does not rely on outdated approach, changes the way of thinking, and permits one to develop a time integration scheme that is based on cloud physics and the theory of finite-difference schemes. Anyway, two choices are better than one, and scientists can choose which approach better fits their "needs". We would like to promote a scientific approach based on very strict rules of applied mathematics as opposed to the approach based on a willingness to have some FORTRAN code that does not blow up in a course of a host model integration.

5. I would like to see an analysis of exactly what goes wrong when τ_{max} is exceeded. "Mass-conserving" schemes maintain non-negative water and the ratio between rates for individual processes stays constant across all $dt > \tau_{max}$ (since rates are just rescaled). Process rates will decrease, however, to ensure that all water is just depleted at the end of the step. So is the issue that microphysics will play a diminished role at longer timesteps? Perhaps you could test this by running CAM or WRF with various dt and looking at the net microphysical rate compared to that of other processes.

Our analysis provides a proof of nonexistence of a unique positive-definite stable numerical solution in an explicit Eulerian time integration framework for the differential equations that govern warm rain microphysical processes for microphysical environmental conditions and arbitrary chosen timestep for which $N_{sm} > 1$. Additionally, our analysis shows that growth rates cannot be "rescaled" because these rates do not exist for $\tau > \tau_{max}$. Moreover, for $\tau > \tau_{max}$ microphysical processes are not governed by explicit Eulerian finite-difference equations as it is assumed in a non-well-behaved EEBMPC. In summary, our analysis forbids the extrapolation of existence of a positive-

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definite explicit Eulerian numerical solution to a time interval that is greater than that given by the general SM-criterion. Violation of this "rule of thumb" in an explicit Eulerian time integration framework is a quintessence of the "mass conservation" technique that assumes that "adjusted" growth rates are applicable at the time interval where numerical solution does not exist. Only understanding of the fact that a numerical solution does not exist on an arbitrary chosen time interval is enough to reject utilization of the "mass conservation" technique, and any additional proof for this rejection is not needed including any tests by "... running CAM or WRF with various dt and looking at the net microphysical rate compared to that of other processes".

Minor comments:

1. The term "Community models" is odd since I think you're just referring to the models put out by NCAR rather than a certain type of model. Or maybe you're including the GFDL model as well? And if so, is the GFDL microphysics the same as the CAM microphysics? If so, your comments about the different GCMs is misleading. In any case, I think your comments are applicable to all current mesoscale and global models so perhaps you should say this instead of "community models".

We do not include GFDL model, whose microphysics is different from CAM microphysics (analysis of these differences is out of the scope of our paper). However, numerical treatment of cloud water in GFDL microphysics based on the "mass conservation" technique remains similar to that used in EEBMPC with prognostic equations, which are discussed in our paper.

On page 1404 (lines 15-16) one can read "... that govern warm rain formation processes in microphysics packages in Community models (CAM and WRF)". Additionally, on page 1406 (lines 16-17) we restate "This feature of BLK schemes implemented in Community models (CAM and WRF) could lead to...". We use word "Community" in the same sense as it is used on official WRF and CESM sites.

We agree that our analysis is applied to almost all BLK scheme whose numerics is based on an explicit Eulerian time integration framework in current regional and global models.

2. p. 1407 L 10: "*positive X(t) and *Y*(t)*"

Thanks. We should correct it.

3. Fig 1-4: *how did you calculate these? By running the code offline, or by actually writing out formulae for how things would change?*

It is explained on page 1412: " Thus maximal time step permitted to keep EE time integration scheme stable and positive definite corresponds to

$$N_{sm} = 1 \quad (15)$$

Maximal time steps calculated according to expression (15) for TAO, THOMPSON, MORRISON, and WSM6 WRF BLK schemes as functions of Q_c and Q_r for two different droplet concentration $N_c = 10 \text{ cm}^{-3}$ and $N_c = 100 \text{ cm}^{-3}$, which are used as proxy for "maritime" and "continental" clouds, are shown on Figs. 1 and 2 and Figs. 3 and 4, respectively". It should be understandable that N_c values are used to designate an order of magnitude only. When dependence of particular formula on N_c is known one could easily make correction to the time step shown. Expression (15) permits calculations of maximal timesteps using values on X-axis and those shown on the top of each Figure. This comment indicates that P. Caldwell missed additional important point on the same page 1412 "... The set of these four figures represents a simple yet powerful tool to analyze behavior of a BLK microphysics scheme. Because utilization of a single column model (SCM) is a conventional way to validate new microphysics parameterization, observations (vertical profiles of cloud water mixing ratio, rain mixing ratio, and cloud droplet concentration) and data from Figs. 1–4 can be used to analyze theoretical vertical profiles of SM-criterion". For example, it means that if P. Caldwell is

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responsible for SCM intercomparison setup for which initial profiles of liquid water mixing ratio and droplet concentration are known from observations (or range in which they might change as well as expected rain mixing ratio) he would ask participants to run their models with a timestep that does not exceed the timestep given by (15). Results from this run accompanied with results of an additional run with the timestep routinely used for particular SCM integration will show how the "mass conservation" influences on accumulated precipitation on the surface. If the timestep is chosen according to expression (15) or even less it would mimic performance of a well-behaved EEBMPC.

4. 10 cm^{-3} is extremely low, even for pristine marine conditions. 100 cm^{-3} is more reasonable for marine conditions, and perhaps you should use a larger value for land?

Please see our response to previous comment.

5. I think when you say "bounded" you mean that cloud water should decrease and rain water should increase while the sum of the 2 stays constant and $q_c \geq 0$. This is different than the typical usage where the upper and lower bounds can be arbitrarily large.

P. Caldwell is right with only one exception. The sum of these two non-negative numbers is constant and does not exceed its initial value on each timestep. We use boundedness in a sense this word is usually used in the theory of finite-difference schemes.

6. I am pretty sure the Morrison schemes in CAM and in WRF are very different. Do you mean that both use the same autoconversion and accretion formulations? If so, you should clarify.

We have never claimed that these schemes are the same. In our paper we analyze numerics of these schemes. On page 1426 one can read "... both CAM (Gettelman et al., 2008) and GFDL AM3 GCM (Salzmann et al., 2010) utilize diagnostic equations for

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precipitating hydrometeors, but numerical treatment of cloud water remains similar to that used in EEBMPC with prognostic equations, which is discussed above". Not only KK2000 autoconversion and accretion growth rates formulae are used in both WRF and CAM, but also numerics applied for cloud water based on the "mass conservation" technique is similar.

7. I don't think you ever clearly define "non-well behaved" and use "well-behaved", etc before defining them.

Please read our text on page 1423 (line 25) "... Depending on the validation of SM-criterion in Explicit Eulerian Bulk Microphysics Code (EEBMPC), we introduce a definition for well-behaved EEBMPC, conditionally well-behaved EEBMPC, and non-well-behaved EEBMPC. In well-behaved EEBMPC, SM-criterion is always validated and satisfied...". In the revised paper we will include a better description of our classes.

8. There are a lot of unnecessary acronyms.

We have revised the paper to reduce acronyms used where possible.

*9. p. 1423 L 18: All of these schemes *are* positive definite because of their "mass conserving" aspect. They are just not realistic.*

We agree that these schemes are not realistic if $N_{sm} > 1$. Moreover, these schemes cannot be considered a numerical scheme at all. These algorithms have nothing in common with the numerical solution for the governing differential equations because, as our analysis shows, this solution does not exist for $\tau > \tau_{max}$.

10. p. 1424 top: As noted above, the SM criterion does not ensure that the numerical solution is correct.

We disagree with P. Caldwell. At first, an answer to a question "What does it mean to find a numerical solution of the initial value problem (IVP) as it was formulated in

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Section 2 and Section 3?" should be recognized.

It should be understandable that a "time interval", on which a solution for our IVP exists, is an integrative part of this solution. As our analysis shows in an explicit Eulerian framework a unique stable positive-definite numerical solution on a whole time interval determined by a host model exists if $N_{sm} \leq 1$. Limiting case ($N_{sm} = 1$) permits calculate a maximal time interval, on which this unique solution exists (all cloud water is depleted). For this particular situation, an answer to a question "what is a numerical solution?" should be "AFTER τ_{max} cloud water is zero, and rain water is equal to a sum of initial liquid water content and initial rain water content". For a timestep greater than that given by the SM-criterion a unique bounded stable positive numerical solution for our IVP in an explicit Eulerian time integration framework does not exist. The condition $N_{sm} \leq 1$ imposes an additional restriction on a timestep permitted for a host model integration. Otherwise, a treatment of microphysics that is based on the governing differential equations used in BLK schemes constitutes an ill-posed mathematical problem.

11. p. 1425, L 10: I think mass-conserving schemes will actually be very stable because they just zero out any unreasonable liquid. In fact, they probably get more stable at longer timesteps.

We really do not understand what an "unreasonable liquid" means. It implies there should be a "reasonable liquid". What is that?

As our analysis shows, if $N_{sm} > 1$ these schemes cannot be stable nor non-stable because a unique stable positive-definite numerical solution for the governing differential equations does not exist in an explicit Eulerian framework. It makes no mathematical sense to discuss stability of a non-existent solution (please see our response to Reviewer 3, specific comment #6).

12. p. 1426: I don't see how diagnostic precipitation combined with other prognostic

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terms is such a problem. Can you explain?

Detailed analysis of numerics of a prognostic approach for "cloud water" combined with a diagnostic treatment of "rain water", which has never been done, is provided in our separate paper that is in preparation for publication. We would provide P. Caldwell with a reference when it is available.

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