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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

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

Reviewer's comments are in *italic*

This paper is a critique of the limiters in current microphysics schemes used in mesoscale and global climate models. The paper shows using simplified analysis how a maximum stability time step should be defined, and shows how microphysics schemes may violate it.

This paper is an analysis of the numerics used in BLK schemes. We do not criticize, but we analyze what has been done by others. One of the main points of this paper is the proof of the nonexistence of a stable positive definite explicit Eulerian numerical solution for the governing differential equations under consideration if the microphysical environmental conditions and a timestep arbitrary chosen for model integration lead to violation of the analytically derived condition ($N_{sm} \leq 1$) that remains valid regardless of the parameterization used for autoconversion and accretion.

The paper is too dense and too long, and makes unjustified claims. Major sections are highly duplicative and need to be condensed and better related to each other as noted in the specific comments below. The paper takes a myopic focus on the stability of the schemes but does not show that the problems that occur significantly affect the desired solutions on the large scale: the impacts may be only for extreme events that rarely occur or that the codes are not designed to treat, because they represent cases where convective adjustment takes over.

We have revised the paper to make it clearer and brief where ever possible. Our major sections are not "duplicative" because each section has a different meaning as explained in our detailed response to comment #4 of Reviewer #2 who also raised a similar point. Our conclusions are valid for a broad range of microphysical environments in the real atmosphere regardless of the parameterizations used for autoconversion and accretion and "convective adjustment". Moreover, as opposed to the reviewer's conclusion, in our paper a simple microphysical criterion that determines the non-existence of "desired solution on the larger scale" is derived. If for some reasons (for example, due to "convective adjustment") the SM-criterion is respected (as shown in our Figs. 1-4) conditionally well-behaved EEBMPCs would mimic well-behaved EEBMPC performance. However, if a host model passes to the BLK scheme microphysical characteristics and timestep used for the host model integration whose combination lead to a violation of the condition that $N_{sm} \leq 1$, a conditionally well-behaved EEBMPC inherits all deficiencies of a non-well-behaved EEBMPC. However, even thought it is out of the scope of our paper to demonstrate the problems at larger-scale (this is the focus of ongoing research for our future paper) we performed a simple check of the SM-criterion in the CESM version released this year. We downloaded and installed the latest publically accessible CESM version. We run the model for one timestep ($dt = 1800$ s). For precipitating hydrometeors in CESM, a diagnostic treatment is used in the general framework of the MORRISON scheme (as summarized in Morrision and Gettleman, 2008). However, the treatment of cloud water remains the same as analyzed in our paper. This prognostic cloud water/ice plus diagnostic rain/snow scheme uses two equal substeps ($\tau = 900$ s) to advance the microphysical equations in an explicit Eulerian framework. Our check consists of a simple FORTRAN "if-statement" added to the microphysics code supplied with the CESM distribution. This statement checks if the SM-number is greater than one for grid points where the cloud water mixing ratio is greater than zero. In pre-configured CESM immediately available after installation, total amount of grid points where condensation (source of cloud water) might occur is equal to 144x96x30=414720 (2.5x1.9x30 vertical levels). During the first timestep the total number of points where the SM-criterion is violated is equal to 128406. It means that in about 30% of the grid points, the SM-criterion is not respected, the conditionally well-behaved EEBMPC implemented in CESM becomes non-well-behaved EEBMPC, and the need to use "virtual" cloud water mixing ratio $\mathrm{q}_{\mathrm{cv}}^{\mathrm{n}}$ and "virtual" rain water mixing ratio $\mathrm{q}_{\mathrm{rv}}^{\mathrm{n}}$ occurs (as explained in more details in our response to specific comment #16 below). Our observation is verifiable, and can be easily done. By operating with "virtual" reality at different altitude levels in different geographical locations the conditionally well-behaved EEBMPC implemented in CESM renders an uncertainty to the calculation of the precipitation amount and its temporal and spatial patterns.

The paper is not suitable for publication in GMD in its current form, and would need a major revision to be suitable. Finally, the paper is not written in grammatically correct English and needs some smoothing out of the language (particularly missing articles). This is not a fatal flaw, but needs to be corrected before the paper is accepted.

We have revised the paper taking into account both this and the two other reviewer's concerns and hope the revised paper, edited for grammatically correct English and conciseness, is suitable for GMD.

The basic tension is that the authors seek to make the differential equations dominate over mass and energy conservation. This might work for some scales, and it certainly might help in some cases in global models, but it is not clear that this is a major problem, or the solutions are appropriate.

The only conservation law that is relevant in the context of our paper is mass conservation. This conservation law is expressed by the differential equations (3) and (11) as well as by the finite-difference equations (page 1416, line 9 and page 1420, line 10). For any well-behaved EEBMPC, mass is conserved by definition, and any artificial "mass conservation" is not needed. Differential equations cannot dominate "over mass and energy conservation". It is completely unclear what the reviewer means by writing that "... this might work for some scales and it certainly might help in some cases in global models...". Conservation laws, as a rule, are expressed by differential equations. One of the points in the paper is that "appropriate" solutions do not exist if the SM-criterion is violated.

To be publishable in GMD, this manuscript would have to be much shorter and less duplicative, and not make speculative statements, but provide more justification based on different case studies and even global analyses.

Please see our responses above. This paper analyzes the numerics used in microphysics schemes. A separate paper will examine the implications of these findings through global simulations and case studies. We have included these statements in our revised paper (Please see our response to Reviewer 2, Comment #1).

Major comments:

1) The authors do not justify their assertion that the problem of adjusting microphysical process rates to assure mass and energy conservation is an artificial forcing agent. If mass and energy were NOT conserved, this would be a forcing agent.

As our analysis shows an "adjustment" can not be done, because this "adjustment" implies that a positive definite stable explicit Eulerian numerical solution exists even if $\mathsf{N}_{\mathsf{sm}} > 1$. There is no mathematical foundation for this assumption that is simply incorrect. The utilization of this mathematically incorrect assumption lead to the necessity of dealing with a "virtual" microphysical reality through algebraic manipulations ("adjustment"). These artificial manipulations itself make the numerics in BLK schemes based on "mass conservation" technique to be a hidden climate forcing agent. However, we recognize that the term hidden forcing climate agent needs to be further substantiated. 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 $\mathrm{W\,m^{-2}}$ change in TOA radiation. We have included these statements in our revised paper and also have modified our statements on both pages 1406 and 1426 to remove the reference to hidden climate forcing agent but suggest that the errors from numerics could be large enough to have an impact on radiation (e.g. the 1 $\mathrm{W\,m^{-2}}$ 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.

2) There is no general discussion of the magnitude of the effect. As such, the attacks on the suitability of the approach are mostly a red-herring. These claims (repeated several times) should be backed up with analysis or removed. It is not clear to this reviewer that the simplified alternatives proposed are any better. Yes, there may be cases where the process rates are wrong. But you have never shown that this will matter for the hydrologic cycle.

Please see our response above to General comment #2 as well as comment #2 to reviewer #2 regarding the magnitude of the effect. Our goal is to provide the modeling community with the minimal knowledge needed to implement BLK schemes in atmospheric models. From this perspective, section 4 is a minimal "to do list". All tasks in this list should be accomplished before the implementation of a BLK scheme into more complicated models. Additionally, our goal is to demonstrate that collecting different formulae for growth rates due to numerous microphysical processes within a FORTRAN code (as it is thought this process constitutes a BLK scheme development) is not a substitute for the theoretical analysis of numerics that is used to advance governing microphysical equations. "Process rates" cannot be "right" or "wrong" in the context of our paper because our conclusions remain valid regardless of the different techniques that have been used for their formulations.

3) This paper zeros in on criticizing a particular set of assumptions in microphysics schemes without a discussion of the broader context. For example, the process rates you are looking at are empirical, and what if they are simply wrong and inappropriate for the conditions of the state at a point? You are simply making the process rate dominant. It is not clear this is any better or worse than the mass conservation approach given simultaneous calculation of process rates.

The question if "mass conservation" technique is "any better or worse" is hard to follow because our analysis shows that if $\mathsf{N}_{\mathsf{sm}} > 1$, a positive definite numerical solution cannot be found in an explicit Eulerian time integration framework, regardless of the number of microphysical processes under consideration. If additional non-condensational microphysical processes compete for available cloud water the SM-criterion for warm clouds would be more restrictive. We are not "making process rate dominant", we even do not understand what the reviewer means when stating "in a broader context". In our paper, we analyze the numerics in publicly accessible microphysics codes.

4) As also noted below, just focusing on warm rain ignores other process rates in the microphysics that complicate (and may buffer) the equations.

This analysis will be extended to other cloud types and we have included this statement in our revised paper. Our focus was on "warm rain" processes. In an explicit Eulerian time integration framework the SM-criterion remains valid regardless of the amount of different non-condensational microphysical processes that compete for the available cloud water. However, if other processes are included, the SM-criterion would be only a necessary but not sufficient condition for positive definiteness and stability. We would like to add that the inclusion of a new microphysical process or the modification of description of an existing one is not straight-forward. The stability analysis of numerics in a BLK scheme should be redone if any modification is introduced. The minimal "to do list" for this stability and positive definiteness analysis is provided in our section 4. We will include these additional statements in our revised paper to highlight the role other processes play.

Specific comments

1) Since the schemes here are used in many models the term "community models" here is awkward. Why not just refer to "bulk microphysics schemes commonly used in weather (mesoscale) and climate (GCM) models".

We use the word "Community" in the same sense as it is used on the official WRF and CESM web sites.

2) P1406, L4: "additional artificial concentration adjustment" is applied: Where? Is this specified?

In the MG08 scheme implemented in CESM whose source code is publicly available (FOTRAN file cldwat2m_micro.F), lines No. 2239-2240 of the code are used to imple-

ment the process of the so called "concentration conservation" to avoid negativeness of cloud droplet concentration due to autoconversion and accretion processes (lines 2241-2244 of the code are used to "adjust" droplet concentration growth rates due to other microphysical processes). The need to apply additional artificial "concentration adjustment" to keep hydrometeors' sizes within "known physical sizes" is explained in our detailed response to comment #12 and #13 of Reviewer #1 who also raised a similar point. This additional artificial "concentration adjustment" can be found in many places of the code. For example, droplet "concentration adjustment" is given by lines 3210-3243.

3) P1406,L10: How is this an artificial forcing agent? Time integration limits solutions to be physically realistic (non-negative mixing ratios, consistency between mass and number). This is not a forcing agent.

Time integration framework can not "limit solution to be physically realistic". This statement is difficult to understand. However, timesteps used to solve finite-difference equations in particular time integration framework can be limited due to mathematical and physical reasons. Our paper shows how it should be done for warm rain processes in explicit Eulerian time integration framework. Disrespect of limitations imposed on timestep makes the behavior of the whole system governed by the differential equations under consideration highly uncertain due to incorrect numerics that acts as a hidden climate forcing agent by itself. The sense of using incorrect numerics is introduction of "virtual" reality as explained in more details in our response #16 below.

4) P1406, L19: "Could lead to erroneous conclusions" regarding different processes and their relative magnitudes. This needs to be specified.

This sentence has been revised to state that numerical errors could affect the output obtained from microphysics processes used in BLK schemes. More details are given in our comment #16 below.

5) P1406, L23: What does SM refer to? Stability and What?

To refer to this criterion we call it SM (Sednev & Menon) for want of a better term. We are open to other suggestions.

6) P1409: Eq 9 and 10: why neglect all the other microphysical processes here? The major difficulty of microphysics schemes and inconsistency is having to calculate all microphysical processes simultaneously. There are typically a lot more terms here. At least make a mention of that and how your solutions would approximate it or are illustrative.

The reviewer raises an important point. Major difficulties in obtaining numerical solutions for prognostic governing differential equations are 1) to prove theoretically that a unique numerical solution exists, 2) to identify theoretically the parameter space for which this solution exists, 3) to demonstrate theoretically that the time integration scheme, which is used to advance finite-difference equations, obeys conservation law (if it exists), is stable as well as positive definite (if dictated by physics). The theoretical analysis of finite-difference equations used in BLK microphysics schemes is a challenging multi-step task. It is not surprising that papers on this topic are missing. To tackle this task we decided to start with the analysis of warm rain formation processes because of their crucial importance. It is worth noting that warm rain processes to a great extent determine the amount of accumulated precipitation and its spatial and temporal patterns. To complete this challenging task our analysis is being extended to include additional processes (for example, a paper devoted to the analysis of the numerics of rain formation processes in mixed-phase clouds is in preparation) to demonstrate how inclusion of additional processes influences the general scheme behavior. We have noted these additional points in the revised paper.

7) P1409, eq 12: Again, what about other processes? Because of this, your equation

is not a necessary condition in the schemes, it is only part of it. There are other source and sink terms occurring simultaneously.

Inequality (12) is the NECESSARY condition (for warm clouds) that remains valid for any parameterization (linear or nonlinear) for autoconversion and accretion in an explicit Eulerian time integration framework. If other processes are included it is thought that this condition is a necessary but not a sufficient in an explicit Eulerian time integration framework.

8) P1410, L0-8: There are multiple correct solutions for these differential equations based on a balance of processes. I do not think the characterization you have provided here is fair. Also, the wording here is very strange. Are all the schemes you list EEBMCs? Or are only some of them? It is not clear what you mean. The categories could be better described. 'Well-behaved' = checking for timestep limits.

There is one and only one solution for the finite-differences equations in an explicit Eulerian time integration framework if the SM-number is not greater than one! (EEBMPC stands for Explicit Eulerian Bulk Microphysics Code). We will include a better description of classes as recommended.

9) P1410, L10: Why would a well behaved EEMBC not also need to have a mass conservation limiter due to other simultaneous processes?

Because according to the definition a well-behaved EEBMPC has to use only stable and positive definite schemes. In an explicit Eulerian time integration framework, it is equivalent to not violating mass conservation, and additional artificial "mass adjustment" is not needed.

10) P1412: Figs 1-4: The problem seems to be most acute for extreme conditions (N_c=10 cm^{-3} is pretty low) and large Q_r and Q_c. How often are these conditions found in the atmosphere? Certainly in the large scale models, bulk microphysics schemes *are not applied to deep convective clouds where this problem is mostly seeming to occur (large updrafts would be needed to produce* $\mathrm{Q_c}$ *and* $\mathrm{Q_r}$ *in excess of 1-3* $\mathrm{g\,kg^{-1}}$*). Thus this problem would not occur there. Also: Why do you only show 4 schemes? Why not show the Rasch, Kessler and Lin schemes on the figures?*

Figs. 1-4 show ranges of $\mathrm{Q_c}$ and $\mathrm{Q_r}$ in the real atmosphere, as well as of two values (low/high) of droplets concentration that designate only an order of magnitude. (Also note that Hoose et al. (2009, GRL) discusses a range of Nc found in a variety of locations and a value of Nc of 10 cm-3 is acceptable as an indicator of values in clean marine environments). The maximal time step in our figures can be easily evaluated for different concentrations by multiplying the time step shown by "correction" coefficients because the dependence of a particular formula on concentration is known. Keeping this in mind a definition of conditionally well-behaved EEBMPC is introduced in our paper. The conditionally well-behaved EEBMPC mimics well-behaved EEBMPC behavior if the SM-number is not greater than one. If at a particular vertical level in a particular geographical location at a particular time in a GCM run the SM-criterion is not violated (even if it has never been checked), a conditionally well-behaved EEBMPC would show correct performance. However, if for the same column at a different vertical level with a different $\mathrm{Q_c}$, $\mathrm{Q_r}$, and $\mathrm{N_c}$ the SM-criterion is not respected it would lead to a need to "adjust" growth rates. As a result we could find ourself in a situation where growth rates at two different vertical levels (probably adjusted) are calculated using different formulas. These undesired situations should be avoided. It is not clear how often it happens in long GCM runs. Because a well-behaved EEBMPC has never been used in "real" GCM simulations there is no "benchmark" information regarding, for example, spatial and temporal accumulated precipitation patterns. Based on our analysis and experience we expect to have significant differences.

Please note that we provide maximal timesteps for only four schemes in our figures for presentation reason as a figure with seven lines looks crowded. The timesteps for the seven schemes are provided in Table 1.

11) P1413, L17: Why should the schemes assume linearity? If they are non-linear, particularly for lots of processes, then would this analysis still hold? The non-linear nature and need for integration is the basis of how most of the explicit schemes work? I am not sure linearization is appropriate.

The main assumption under which an explicit Eulerian time integration framework is based is that all right hand sides (RHSs) terms in the finite-difference equations are known. It means that growth rates (that are nonlinear with respect to $\mathrm{Q_c}$, $\mathrm{Q_r}$, and $\mathrm{N_c}$) have to be treated as "frozen" during the microphysical timestep. These rates are not functions that depend on time during microphysical timestep but are known constants whose values are calculated at the beginning of the microphysical timestep and can not be changed. "Linearized" in the context of an explicit Eulerian framework means "unchanged during the timestep". That is why known growth rates can not be adjusted! Any "adjustment" is a violation of the main assumption of an explicit Eulerian time integration framework.

12) P1414,eq 19 and 20: Can you explain where the exponents come from for $\mathrm{Q_c}$ *and* $\mathrm{Q_r}$ *(a reference to common formulations perhaps).*

Because we use a general description for any PAUTO and PACCR, whose analytical representation is not known, Equations (19) and (20) indicate how linearization is done. The exponent comes from identity

$$\mathrm{PACCR} \;=\; C_a^0 Q_r^0 Q_c^0 = \mathrm{PACCR}(\mathrm{Q_c^0}, \mathrm{Q_r^0})[\mathrm{Q_c^0}]^{-1}[\mathrm{Q_r^0}]^{-1} Q_c^0 Q_r^0 = \mathrm{PACCR}(\mathrm{Q_c^0}, \mathrm{Q_r^0})$$

13) P1416: How is equation 40 different than equation 27? Sections 4.1 and 4.2 have a lot of duplicate algebra. Can you synthesize and explain it better?

In subsection 4.1 and 4.2 we consider different types of equations that are differential-difference and finite-difference equations, respectively. There is no "duplicate algebra" at all. These subsections cannot be "synthesize" because they have completely different meaning and are absolutely necessary in the context of our paper. Please see our response to reviewer #2 under comment #4 that explains it in more detail.

14) P1417, L6: Where does $\lambda_{1,2}$ come from? The reader (like me) may not be familiar with your matrix notation. Please explain.

It comes from definition of stability routinely used in stability analysis. More details can be found in any textbook on numerical methods used to solve differential equations.

15) P1418: Equation 51: how does this relate to equation 40 and 27? How is this valid if you add 8 other process rates?

Please see our detailed response to reviewer #2 under comment #4.

16) P1419: Section 4.4 duplicates what you have gone over in 4.1 and 4.2 again. Perhaps they can be skipped in favor of this? What is worse, you don't refer back to the earlier sections and how the equations are related. You use the same language (e.g.: P1420,L11-13) but never acknowledge having used it before. Strange. How does this relate to the earlier sections? Perhaps combine them?

Please also see our detailed response to reviewer #2 under comment #4 since that reviewer also posed a similar question that explains why Sec. 4.1, 4.2 and 4.3 are different.

Subsection 4.4 does not duplicate subsections 4.1 and 4.2. On the contrary, it shows how general considerations discussed in subsections 4.1 and 4.2 (that are valid for any parameterization of autoconversion and accretion processes) should be applied to the analysis of numerics for a particular BLK scheme (MORRISON scheme) in an explicit Eulerian time integration framework when analytical representations of functions PAUTO and PACCR are known. First, we provide the differential equations (52)-(53) for this scheme where the autoconversion PAUTO and accretion PACCR rates are given by KK2000 formulae. Second, we provide the finite-difference equations in an explicit Eulerian time integration framework (54)-(55) for the MORRISON scheme (these finite-difference equations have not been provided and discussed by authors or implementors of this scheme). Third, we show that PAUTO and PACCR are known constants whose values are calculated according to (56)-(57) at the beginning of the microphysical timestep. These constants cannot be changed according to the basic assumption of an explicit Eulerian framework (RHSs of differential equations are assumed to be known). Fourth, we derived the SM-criterion (positive-definiteness condition) that for the MORRISON scheme is expressed by (60). According to our stability analysis in subsection 4.3, for an explicit Eulerian scheme the SM-criterion is also a stability condition. As explained above any attempt to use a timestep that is greater than that given by (60) makes no mathematical and physical sense in an explicit Eulerian time integration framework. Because the SM-criterion is never checked in the MORRISON scheme we classify this code as belonging to conditionally well-behaved EEBMPC class. However, if condition (60) is violated for a particular "set" of $\{q_c, q_r, N_c$, and $\tau\}$ passed by a host model, the FORTRAN code for the MORRISON scheme would become non-well-behaved EEBMPC. An attempt to avoid negativeness of q_c^{n+1} calculated according to the finite-difference equation (58) by applying a "reduced" autoconversion AAUTO and accretion AACRR given by (61) and (62), respectively, that act during timestep $\tau > \tau_{max}$ ("mass conservation" technique) is artificial and has nothing in common with the numerical solution for differential equations (54)-(55) using the explicit Eulerian finite-difference equations (58)-(59) that have no positive-definite and stable solution for $\tau > \tau_{max}$. If the SM-criterion is not respected, a non-well-behaved EEBMPC in the MORRISON scheme creates a virtual microphysics reality characterized by a "virtual" cloud water mixing ratio (q_{cv}^n) and rain water mixing ratio (q_{rv}^n) that are used instead of a "real" cloud water mixing ratio (q_c^n) and rain water mixing ratio (q_r^n) supplied by the host model. These virtual numbers can easily be calculated using the following procedure. If for input $\{q_c^n, q_r^n, N_c^n$, and $\tau\}$ supplied by a host model $N_{sm} > 1$, artificial "adjusted" rates AAUTO and accretion AACRR are calculated using formulae (61) and (62). Then a system of two equations for "virtual" q_{cv}^n and q_{rv}^n derived by a) substitution of q_{cv}^n and

$\mathrm{q}_{\mathrm{rv}}^{\mathrm{n}}$ instead of "real" $\mathrm{q}_{\mathrm{c}}^{\mathrm{n}}$ and $\mathrm{q}_{\mathrm{r}}^{\mathrm{n}}$, respectively, in (56)-(57) and b) replacement of PAUTO and PACRR with AAUTO and AACRR in (56) and (57), respectively, has to be solved:

$$\begin{aligned} q_{\mathrm{cv}}^{n} c_1 [N_{\mathrm{c}}^{n}]^{-1.79} [q_{\mathrm{cv}}^{n}]^{1.47} &= \mathrm{AAUTO} \\ q_{\mathrm{cv}}^{n} c_3 [q_{\mathrm{cv}}^{n}]^{0.15} [q_{\mathrm{rv}}^{n}]^{1.15} &= \mathrm{AACCR} \end{aligned}$$

The remarkable feature of these "virtual" solutions $\mathrm{q}_{\mathrm{cv}}^{\mathrm{n}}$ and $\mathrm{q}_{\mathrm{rv}}^{\mathrm{n}}$ is that "virtual" SM-number for the MORRISON scheme ($\mathrm{N}_{\mathrm{smv}}$) defined as

$$\mathrm{N}_{\mathrm{smv}} = \frac{\tau\{c_1[N_c^n]^{-1.79}[q_{\mathrm{cv}}^n]^{2.47} + c_3[q_{\mathrm{cv}}^n]^{1.15}[q_{\mathrm{rv}}^n]^{1.15}\}}{q_c^n}$$

is always equal to one. Physical meaning of this is that "real" cloud water is completely depleted by "adjusted" rates acting during timestep τ, and, as assumed, the problem of negative of cloud water mixing ratio on the next timestep is eliminated. The artificial growth rates, for whose calculation "virtual" $\mathrm{q}_{\mathrm{cv}}^{\mathrm{n}}$ and $\mathrm{q}_{\mathrm{rv}}^{\mathrm{n}}$ were used, are passed to a host model for post-processing analysis.

17) P1418, L17-24: This is pretty much pure speculation. Do you show an example of where these schemes violate the condition in practice, and how it might affect solutions in a case? Note comments above that these schemes may not be treating the high LWP and Rain WP cases you are concerned with because they work in conjunction with moist convective adjustment.

An example how often the SM-criterion is violated is provided in our response under General Comments #2. Additionally, disrespect of the SM-criterion is not determined by the values of LWP and Rain WP (RWP) that are vertical integrals of LWC and Rain WC (RWC), respectively. Violation of the SM-criterion might occur for different combinations of LWP (high/low) and RWP (high/low) depending on particular values of LWC and RWC at particular altitude and vertical resolution.

18) P1421, L7-13: again, is this a real issue or a red herring? When would it occur? What happens when you consider other process rates in your equations that are supposed to act simultaneously?

When other processes are included, the SM-criterion should be violated more often because other processes compete for available cloud water as was explained in discussion section. In this case the SM-criterion for "warm cloud" is a necessary but not a sufficient condition.

19) P1422, L5: I do not think your analogy holds. Ensuring mass an energy conservation in a microphysics scheme is not the same as the example you are proposing with advection, since reducing the velocity reduces the total energy (kinetic energy) and violates conservation laws.

Advection cannot reduce or increase kinetic energy. Advection can only redistribute kinetic energy. Total kinetic energy is a constant during advection (with the appropriate choice of boundary conditions). Without defining boundary conditions, the reviewers' statement regarding advection that "reduces kinetic energy" is hard to follow.

We do not focus on energy or mass conservation in our example. We discuss the so called prototype advection equation that is routinely used in stability analysis of different advection numerical schemes. This prototype equation is well known for those who work on the development of numerical schemes and their stability analysis. For a linearized advection equation "advection velocity" is assumed to be constant. The CFL-condition given by the expression on p. 1421 (line 18) is a one-parametric expression with only one unknown τ_{adv} (timestep) whereas two other (advection velocity and vertical resolution) are known constants. Moreover, the CFL-condition is a necessary condition for the advection scheme stability in an explicit Eulerian time integration framework. Any attempt made to extend the numerical solution for a time interval that is greater than that given by (63) will result in an unstable solution because both ΔX_{adv}

and C_{adv} are constants. In the case of warm rain microphysics equations in an explicit Eulerian time integration framework, the SM-criterion determines the maximal time interval for which a positive numerical solution exists, because only the timestep is a variable in (13) whereas others are constants by definition (growth rates cannot be "adjusted" in an explicit Eulerian framework). A willingness to extend the numerical solution for a time interval that is greater than that given by the condition $\mathrm{N_{sm}} = 1$ is a logical mistake.

20) P1424, L10: I am not convinced that your toy models with 2 processes would be sufficiently constrained to not need a mass conservation limiter if you applied this to a full model calculation.

Two processes are enough to understand the general behavior of a more complicated system in an explicit Eulerian time integration framework. If the description of mixed-phase precipitation formation processes is included the SM-criterion would be even more restrictive because additional solid hydrometeors compete for available cloud water. It is clearly stated on page 1426 (lines 26-28). This "toy" is worth tons of computer hours spent on numerical simulations using non-well-behaved EEBMPCs.

21) P1425, L 25: You have not justified that this is a major concern, given the range of rates typical of the large scale stratiform clouds that the models are trying to reproduce. P1426,L20-25: As noted, there is nothing in the manuscript to justify these statements. How does it impact the global water cycle? Can you show that changing the time step to eliminate this problem changes the water cycle? This text has appeared 3 times verbatim in the manuscript (abstract, introduction and conclusions) without justification.

We have revised and removed our usage of the term hidden climate forcing agent as explained earlier. Our Figs. 1-4 show a broad range of cloud water and rain water mixing ratios in the real atmosphere but cloud droplet concentration is a free parameter. If for a particular microphysical environment and timestep arbitrary chosen for model

integration $\mathrm{N_{sm}} > 1$ the utilization of "mass conservation" technique introduces "virtual" microphysical environment that is different from the "real" microphysical environment supplied by the host model. The need to apply an additional artificial "concentration adjustment" technique in conditionally well-behaved EEBMPCs is determined by the need to avoid negative concentration and "to keep water drops within known physical sizes". As a result the microphysics operates with artificial numbers in calculations of precipitation amount, which enters as a boundary condition for ocean, sea ice, and land components of a host model, and supplies "virtual" boundary conditions for these components. The behavior of the whole system then becomes uncertain due to incorrect numerics. Regarding how often it might happen please read our response to General comment #2.

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.

Hoose, C., J. E. Kristjansson, T. Iversen, A. Kirkevag, O. Seland and A. Gettelman (2009): Constraining cloud droplet number concentration in GCMs suppresses the aerosol indirect effect. Geophys. Res. Lett. 36, L12807, doi:10.1029/2009GL038568