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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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First, we thank the reviewer for the review of our paper entitled "Analyzing numerics of bulk microphysics schemes in Community models: Warm rain processes". Additional thanks for crediting us with "... an excellent point or two regarding numerical stability unrelated to dynamics" as well as for the evaluation that "..The criticism regarding "mass conservation" as a technique to prevent negative mixing ratios appears valid".

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Reviewer's comments are in *italic*

1) Overall the authors discuss a very important topic of numerically correct treatment of microphysical processes and make an excellent point or two regarding numerical stability unrelated to dynamics.

We should add that not only numerical stability but also the positive definiteness of an explicit Eulerian time integration scheme used to solve the governing microphysical equations is one of the main points under consideration.

2) However, their assertion of a "hidden climate forcing agent" due to using too long a timestep is entirely misleading alarmist speculation. Without more convincing evidence with actual simulations to show that precipitation changes in statistically meaningful ways due to this issue, I strongly object to these terms in the current manuscript (pg. 1405, ln. 08 and pg.1406, ln. 11).

Our statement that "numerics in non-well-behaved EEBMPCs, which are used in Community Earth System Models, act as a hidden climate forcing agent, if relatively long time steps are used for the host model integration" is based on our analysis, scientific intuition, and knowledge of cloud microphysics and theory of finite-difference schemes as well as experience gained over years while working on microphysics scheme development and running GCMs. 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 needed to maintain stability and positive definiteness of the time integration scheme for various cloud microphysical processes. We recognize that these time steps are too small to implement in large-scale models and also recognize that their significance needs to be established 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 3D baroclinic wave 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 $\mathrm{W\,m^{-2}}$ change in TOA radiation. However, we recognize the implications with using terms such as "hidden climate forcing agent" and 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 $\mathrm{W\,m^{-2}}$ 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.

3) Another weakness in the paper is the lack of considering various limitations that the bulk microphysics code authors may already include in their schemes to avoid physically unrealistic things such as massively large water drops.

Unfortunately, the reviewer mentions unidentified "authors" who might include unidentified "various limitations" in their schemes. It is not clear what the reviewer means. We analyze the FORTRAN codes implemented in models such as WRF or CAM that are publicly accessible. We guess that the reviewer means the breakup process that is the only relevant one in the context of our paper. The only scheme under consideration that treats spontaneous breakup is the MORRISON scheme. This scheme accounts for this process by adding a corresponding term to the governing equation for rain drops concentration . However, the governing equation for Q_r is not affected for physics reasons. The warm rain governing differential equations for cloud and rain mixing ratios remain the same as analyzed in our paper even if spontaneous breakup is included as in the MORRISON scheme. Because "various limitations" mentioned by the reviewer are not known we cannot comment on them. We do not consider this fact as a "weakness".

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4) For instance, most schemes would only reach relatively large value of cloud water content between approximately 1-10 g/kg for a single timestep, because, if the concentration of drops is so low as the authors assumed, then the median drop size would be far larger than 50 microns, which would immediately result in drops converting from the cloud water to rain categories and thereby reduce the cloud water content. This manuscript shows a range of conditions that rapidly becomes impractical in full model simulations as various assumptions in the schemes would typically prevent the occurence of large water contents combined with low droplet concentrations. It is almost as though the authors are trying to exaggerate the severity of this problem by picking a number concentration of droplets of 10 per cc, a condition that no bulk microphysics author would consider wise for widespread usage. As the differences between figures 1 and 3 and Figs 2 and 4 show, the smaller the droplet concentration, the potentially more drastic need for short timesteps (e.g., Fig.2d)

It should be noted that cloud or rain water mixing ratios in our figures never exceed 5 $\mathrm{g\,kg^{-1}}$. Their value of 10 $\mathrm{g\,kg^{-1}}$ is used only as a horizontal axis label. We will correct this problem. Additionally, we have never claimed that one or another value of cloud/rain mixing ratios can be reached during a single timestep. Their values in Figures 1-4 are meant to represent a wide range of water mixing ratios observed in clouds. Moreover, we have never used droplet concentrations $\mathrm{N_c = 10(100)\ cm^{-3}}$ as a proof of our conclusions, but we have used these values of droplet concentration to show an order of magnitude difference in droplet concentration in clouds. In fact 10 $\mathrm{cm^{-3}}$ is an acceptable value for clean marine air masses (see for example Hoose et al., 2009, GRL). It is clearly mentioned on page 1412 (line 3) that these values are used as a proxy for "maritime" and "continental" clouds. Finally, based on our Table 1 we clearly stated that "...WRF BLK schemes under consideration can be used for regional scale simulations if the time step in the host model does not exceed two to three hundred seconds" (page 1411, line 17) keeping in mind ranges of prospective cloud/rain water mixing ratios that usually arise in these simulations and additional restrictions on the timestep imposed by dynamics. Finally, we would like to highlight that if for some reasons rain mixing ra-

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tio reaches 0.3 (0.5) $\mathrm{g\,kg^{-1}}$ for cloud droplet concentration 10(100) $\mathrm{cm^{-3}}$, respectively, for a cloud water mixing ratio range 0.1-0.5 $\mathrm{g\,kg^{-1}}$, the timestep used for large-scale model and SCM integrations should be less than 20-30 minutes timestep routinely used in these simulations. Thus, we disagree with the statement that the "manuscript shows a range of conditions that rapidly becomes impractical in full model simulations as various assumptions in the schemes would typically prevent the occurrence of large water contents combined with low droplet concentrations". This statement accounts only for the relative importance of autoconversion process. If as usual due to "unidentified limitations" water is " immediately converted to rain category" accretion process (that actually does not depend on droplet concentration as parameterized in BLK schemes under consideration) would immediately prevail and set a limit on the timestep permitted as shown, for example, for $Q_c = 0.1\ \mathrm{g\,kg^{-1}}$ and $Q_c = 0.5\ \mathrm{g\,kg^{-1}}$ in Fig. 2 and Fig. 4 for $N_c = 10\ \mathrm{cm^{-3}}$ and $N_c = 100\ \mathrm{cm^{-3}}$, respectively. If for some reasons the SM-criterion is satisfied (even if it has never been checked in the source code) conditionally well-behaved EEBMPCs would inherit the performance of well-behaved EEBMPC. In our Discussion section we stated that "Our analysis shows that source code implementation of single moment (TAO, THOMPSON, and WSM6) schemes and double-moment MORRISON scheme with prognostic treatment of precipitating hydrometeors in WRF use “mass conservation” technique and belong to conditionally well-behaved EEBMPC class if used for cloud-resolving or large-eddy simulations, but they can become non-well-behaved EEBMPC for regional and large scale simulations" (page 1425, line 18). Summarizing, we conclude that we have not tried "to exaggerate the severity of this problem by picking a number concentration of droplets of 10 $\mathrm{cm^{-3}}$".

5) Furthermore, this reviewer rarely sees cases where the model can sustain rain or cloud water contents above 1-3 g/kg for extended periods. Obviously, this is a condition associated with only the most massive updrafts in convective environments, which furthermore requires relatively short timesteps in the first place. So, while the authors make a solid argument of the existence of this issue, notice from Figs. 1-4 how almost

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no scheme has a serious problem for water contents below about 1 g/kg and using timesteps longer than 50 seconds. For that matter, the grid scales needed to produce such high water contents are of order 0.5 to 5.0km which would dictate a model timestep ranging approximately 3-30 seconds. Using Fig. 3 as a relevant example, note how the combination of cloud and rain water contents have to be at their most extreme values before the curves drop below approx 30 seconds. Given my own experience of how infrequently models produce 3-5 g/kg of water content and only do so with extremely high resolution and therefore extremely small timesteps, any assertion by the authors of a noticeable signal in output precipitation that is seriously flawed by approaching a S-M number larger than 1.0 is entirely mis-leading, since such conditions are rarely exceeded.

This comment appears to describe situations that occur in a Cloud Resolving Model (CRM), in which restrictions on time step used for model integration are imposed mainly by dynamics. We mention that in our abstract by stating "...We highlight that source codes of BLK schemes, originally developed for use in cloud-resolving models, implemented in Community models belong to conditionally well-behaved EEBMPC class and exhibit better performance for finer spatial resolutions when time steps do not exceed seconds or tenths of seconds" (page 1404, lines 20-24). Additionally, on page 1410 (lines 16-23) one can read "...The distinguishable feature of conditionally well-behaved EEBMPC is that the SM-criterion is satisfied even if it has never been checked. As used in Cloud Resolving Models (CRM) or Large Eddy Simulation (LES) models with temporal resolution about a few seconds conditionally well-behaved EEBMPC provides a correct solution for governing differential equations because limitation on time step imposed by dynamics is more restrictive than that imposed by microphysics (SM-criterion), and, in fact, “mass conservation” technique is never applied." Moreover, in our Discussion section we restate that "Our analysis shows that source code implementation of single moment (TAO, THOMPSON, and WSM6) schemes and double-moment MORRISON scheme with prognostic treatment of precipitating hydrometeors in WRF use “mass conservation” technique and belong to conditionally well-behaved

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EEBMPC class if used for cloud-resolving or large-eddy simulations, but they can become non-well-behaved EEBMPCfor regional and large scale simulations" (page 1425, lines 18-23). Thus the reviewer's statement that "...any assertion by the authors of a noticeable signal in output precipitation that is seriously flawed by approaching a S-M number larger than 1.0 is entirely mis-leading, since such conditions are rarely exceeded" is not relevant.

6) Once again any concrete evidence of an actual problem in actual simulations is entirely lacking in this manuscript.

We would like to note that our paper is devoted to the theoretical analysis of numerics. But the point is well taken. We have now included additional statements in the revised paper to indicate that forthcoming papers by us will include the relevance of this analysis in simulations with both WRF and CAM. Also included are statements described under response to comment No. 2. The reason that artificial "mass conservation" and "concentration adjustment" introduced more than a decade ago are still in use in BLK schemes is a lack of a) a strict mathematical formulation of the problem under consideration (similar to that given in our Section 2) and b) thorough analysis of numerics used to solve the corresponding governing differential equations (similar to given in our Section 4). Our paper is the first attempt to draw attention to this problem by providing the analytical framework. Subsequent papers will include actual simulations.

7) The criticism regarding "mass conservation" as a technique to prevent negative mixing ratios or number concentrations appears valid. The authors proposed solution related to autoconversion and accretion as sink terms for cloud water is logical and relatively simple to consider and implement. However, I believe a similar approach to treat the far more complex situation of combined 3-species interactions simultaneously with other processes is a far greater challenge. As an example, consider the case for *sink terms for rain such as rain and snow colliding (to form graupel), rain freezing, and rain evaporating. While all of these processes reduce rain, the end result of various processes is different, such as graupel production (or perhaps snow or cloud ice) and source of vapor (in the case of rain evaporating). So how does one apply the proposed technique in the broader sense in order to capture all source/sink terms?*

We would like to note that our paper not only shows that the "mass conservation" technique is an incorrect attempt to make an explicit Eulerian time integration scheme become positive-definite but our paper also reveals that this technique is an incorrect mechanistic extrapolation of both analytical and numerical linearized solutions of governing differential equations for time ranges in which these solutions do not exist (when SM-number is greater than one) if the timestep provided by a host model is greater than that given by the SM-criterion. We also agree with the reviewer's opinion that "a similar approach to treat the far more complex situation of combined 3-species interactions simultaneously with other processes is a far greater challenge". We understand how our approach can be generalized in a way that avoids the necessity of using artificial "mass conservation", and such an approach is under development by us now. On page 1426 (lines 26-28) one can read "...Despite the fact that our analysis is focused on warm rain processes, we highlight that inclusion of ice phase into consideration makes the SM-criterion even more restrictive because additional solid hydrometeors compete for available cloud water". For this case it means that the SM-criterion is a necessary but not a sufficient condition that should be included in any microphysics scheme whose numerics is based on an explicit Eulerian time integration framework. We will include these additional statements regarding challenges in treating additional processes with 3-species interactions.

8) The reason the mass conservation step is found in nearly all bulk microphysics schemes is due to the calculation of a large number of souce/sink transfer rates as if no other processes were occuring simultaneously.

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We highlight that the "mass conservation" technique is not mathematically and physically based whatever maybe the reason for its utilization. "Mass conservation" technique is mathematically incorrect because it extends the numerical solution of governing differential equations for time ranges for which this solution does not exist (as mentioned above). We would also like to note that the problem of negativeness of hydrometeors mixing ratios arises even in CRM simulations (e.g., Ferrier, 1994). We argue that the development and utilization of a"mass conservation" technique was determined not by physical reasons but by the desire to use bulk microphysics schemes originally developed for CRMs in atmospheric models of larger scales. However, the microphysics scheme is very often able to "over-deplete the total resources available" if time steps used for model integration are longer than those routinely used in CRMs. As a solution for this "challenge" mass conservation technique was "invented" for use in a BLK scheme and implemented in atmospheric models whose source codes are publicly accessible. Supposedly, the mathematical and physical consequences of the utilization of this technique were not recognized. However, willingness to ignore these consequences or misunderstanding their use came from the need to run models with larger scales than CRM models that do not "blow up" with longer time steps. Additionally, the "mass conservation" technique is also incorrect from the point of view of cloud physics because its utilization disrespects the parameterization of different microphysical processes (autoconversion and accretion in warm clouds) developed by physicists. In fact, if the SM-criterion is violated, artificial reduced growth rates are used instead of those published in corresponding published papers. The utilization of "mass conservation" technique in non-well-behaved EEBMPCs limits usefulness of these codes for process oriented studies devoted to the evaluation of different formulae for calculation of microphysical growth rates.

9) The creation of those transfer rates come from theoretical and/or laboratory results of a certain process in isolation.

We are aware that different formulations for growth rates due to different microphysical

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processes were derived using different approaches. To develop an analytical formulation for a particular growth rate it is not absolutely necessary to consider "a certain process in isolation". For example, KK2000 formulae for "autoconversion" and "accretion" growth rates were developed by using results of 3D model runs with bin-resolved microphysics when both processes act in unison. All parameterizations for warm rain processes, regardless of approaches applied for their development, share the same common feature. There are no parameterizations that explicitly depend on a timestep used for a host model integration. As discussed earlier an undesirable feature of the "mass conservation" technique is that it disrespects parameterizations developed by physicists as our formulae (61)-(62) show.

10) As such, one process does not "know" that another process is competing for the same resource (vapor for instance) and, therefore, a final check is done to "re-balance" the various terms in the event that many processes combined would "over-deplete" the total resources available. Basically, the number of source/sink terms operating in unison is large and interactions may be entirely non-linear.

All microphysical process are not discussed in the context of our paper. However, we agree that condensational process in real clouds is very important. We would like to note that the numerical treatment of condensational processes in mixed-phased BLK schemes needs reevaluation. We would also completely agree with the first statement (omitting water vapor) if a phrase "in clouds" is appended at its end. However, in an explicit Eulerian time integration framework this statement is incorrect because non-linear growth rates of microphysical processes are known constants at the beginning of each microphysical timestep and can not be changed during this timestep. In the context of our paper the need to "re-balance" (reduce) microphysical growth rates that "operate in unison", "compete for the same resource (cloud water)", and "over-deplete the total resources available (cloud water)" when the SM-criterion is disrespected is due to incorrect numerical implementation. There is no physical reason for such a "re-balance" to occur.

11) So the authors choosing of only 2 processes (autoconversion and accretion) is a great simplification of a far greater problem.

We agree that processes of precipitation formation in real clouds are extremely complicated, and their treatment in numerical models is a challenging task. We decided to initiate a discussion regarding the numerics used in BLK microphysics scheme by considering "warm rain" processes because of their crucial role in precipitation formation in clouds of many types. From the microphysics point of view these process do determine the amount of accumulated surface precipitation in many geographical locations all around the globe. It does not mean that ice processes are of less importance if precise knowledge of temporal and spatial precipitation patterns is needed. We do plan to extend our analysis of numerics in BLK schemes to account for additional microphysical processes that occur in these types of clouds. This additional comment will be included in the revised paper.

12) Artificial number concentration adjustments are a fact of life in codes dealing with incredibly small numbers or mass of hydrometeors as compiler optimization of codes is extremely dependent on many factors. Therefore, I see no potential solution that does not involve compiling without optimization which simply will not be an acceptable solution.

There is no doubt that the compilation of a source code with optimization influences the compiled code performance. However, "compilation with optimization" has nothing in common with artificial "number concentration adjustment". The need to apply additional artificial techniques to avoid negative hydrometeors' concentration arises due to inconsistent utilization of the parameterization of mass and concentration growth rates due to microphysical processes and the incorrect numerical implementation of these processes in double-moment BLK schemes.

13) The re-adjustment of number to keep water drops within known physical sizes is

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absolutely necessary in this reviewer's opinion.

We would like to highlight that the lack of a strict mathematical formulation for the system of governing differential equations for double-moment schemes (similar to that provided in our Section 2) determines the necessity of applying additional artificial "concentration adjustment" techniques in these schemes and leads to the reviewer's ultimate conclusion that "re-adjustment of number to keep water drops within known physical sizes is absolutely necessary". Contrary to this opinion we believe that any artificial technique to ”re-adjust” hydrometeors' concentrations is not needed. All one needs is to use a stable positive definite numerical scheme to solve the system of governing equations formulated in an appropriate way. The stable positive definite numerical solution in an explicit Eulerian time integration framework keeps "water drops within known physical sizes" without artificial "re-adjustment". Such a scheme is currently under development by us.

Minor comments:

In many locations in the manuscript, the authors are incorrectly using "tenths" and "hudredths" when they should be writing "tens" and "hundreds." Examples include lines 23 and 25 on page 1404 but there are other places as well. There are numerous unnecessary acronyms.

Thanks. We have made these corrections.

References

Ferrier, B. S.: A two-moment multiple-phase four class bulk ice scheme. Part I: Description. J. Atmos. Sci., 51, 249-280, 1994.

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