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Discussions

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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This paper makes a number of great points. I strongly agree that there is a problem with the philosophy of the "mass conservation approach" which holds that microphysics "lives its own life" divorced from reality. Microphysics should calculate rates which are reasonable approximations to the actual conditions which would be found over the time it was operating, not rates based on the assumption that microphysics was acting alone during that timestep. I also agree that the nonlinearity of microphysical processes makes linearizing over long timesteps a dubious practice. I have a number of reservations about the paper, however, as outlined below.

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.

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.

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

4. p. 1421: If you compute tau_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.

5. I would like to see an analysis of exactly what goes wrong when tau_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 var-

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ious dt and looking at the net microphysical rate compared to that of other processes.

Minor comments:

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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".

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

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?

3. 10/cm3 is extremely low, even for pristine marine conditions. 100/cm3 is more reasonable for marine conditions, and perhaps you should use a larger value for land?

4. 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 qc>=0. This is different than the typical usage where the upper and lower bounds can be arbitrarily large.

5. I'm 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.

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

7. There are a lot of unnecessary acronyms.

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

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

10. 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.

11. p. 1426: I don't see how diagnostic precipitation combined with other prognostic terms is such a problem. Can you explain?

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