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Discussions

Interactive comment on "Numerical issues associated with compensating and competing processes in climate models: an example from ECHAM-HAM" *by* H. Wan et al.

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General comments

The paper contains an analysis of the problems that may arise as a consequence of the indiscriminate use of operator splitting in numerical models coupling fluid dynamical equations to other physical and chemical processes. The paper is clearly written and its results are relevant and of interest to the atmospheric and climate modelling community.

The paper should be published after minor revisions.

Specific comments:

1) The description of scheme HAM1 should be more precise, it is unclear how the limiter is exactly implemented and whether the clipping acts on S** or on S_{t+deltat} or on both. Furthermore, it is not clearly specified if the 95% limiting is also used in scheme 1EP. In general, it would be better to express all these limiting steps by formulae, including appropriate operators like max(S,0) in the definition of the numerical method.

2) The approach that leads to the method defined by equation 15 looks essentially equivalent to what is known in the literature on ODE solvers as the first order Rosenbrock or Euler-Rosenbrock linearized implicit method (see e.g. the book E. Hairer and S.P.Norsett and G. Wanner, Solving ordinary differential equations, Vol 2, Springer, 1987). If this the case, the proposed method should be referred to as a Rosenbrock method.

3) The observation that splitting nucleation from other processes can lead to unrealistic results is contained in paragraph 27 of Jacobson 2002. It would be appropriate to acknowledge this indication, since the authors have referred to this paper anyway.

4) All the methods considered appear to be first order accurate as a whole. For clarity, some explicit comment on the order of convergence of these methods should be added. I understand that switching to a higher order method might not be worthwhile considering the overall efficency constraints and that simple substepping might be more effective, but I believe that if the goal is to use long timesteps (actually, twice as long as those employed in the dynamical core) simple second order methods could also be useful. The authors might consider introducing a further test with a second order method, in order to check whether there is something to be gained by going higher order or not.

5) For completeness, it would be interesting for the reader to show how scheme 3 performs in a box model test like that of Kokkola 2009. Reproducing something like

fig.1 of Kokkola 2009 would be sufficient, possibly plotting the error with respect to the reference solution for scheme 2 and scheme 3.

Technical comments:

1) p. 687: stiffness is neither the only nor the most important issue addressed in the listed papers, this sentence could be reformulated referring more generally to numerical problems arising in this area.

2) p. 688 : Caldwell 2013 is missing in the reference list

3) p. 694 line 2: the authors claim that their 'method 3' is equivalent to a method proposed in Jacobson 2002, but the referred paper contains a large number of discretizations for different processes, it would help the reader to state specifically which formula in Jacobson 2002 one should look at

4) The stability analysis in appendix A is very standard and can be omitted, referring instead to some basic numerical methods textbook.

Interactive comment on Geosci. Model Dev. Discuss., 6, 685, 2013.