Comments on paper "A test of numerical instability and stiffness in the parametrization of the ARPEGE and ALADIN models"

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1 General comments

In this paper the author presents a testing methodology and a specific diagnostic for detecting numerical instabilities due to stiffness in physical parametrizations (co-existence of fast and slow timescales). The diagnostic is simple to code and seems an effective way to expose and pinpoint numerical instabilities which exhibit themselves as oscillations with period of two timesteps.

In my opinion this work is useful for NWP modelers and applicable to a variety of models. The results presented are sufficient to demonstrate the effectiveness of the methodology, however, I do find that the author needs to put effort to improve readability and clarity and explain better the so called stiffness test. Furthermore, I have some doubts if the proposed test is the best way to expose stiffness.

2 Specific comments

- **Section 2.2** The text in the 2nd paragraph gives the impression to the reader that all is done in 2.2 is to apply different numerical schemes (implicit, trapezoidal, explicit) on two different versions (linear, non-linear) of the simple test equation (1) and compare them. It is mentioned that "Figure 3 shows solutions to linear and non-linear form of Eq (1) when the fibrillation test is applied". However, at this point in the paper the testing methodology has not been clearly defined in the context of the non-linear diffusion equation and only a hint has been given (second before the end sentence of section 2.1) which can be misinterpreted. *So, I would invite the author to clearly explain what this test precisely is and how it is applied on the simple non-linear equation. The discussion in the first paragraph regarding how the test is applied on parametrizations is best to be limited to the context of the test equation as this needs to be understood first before a complicated model is tested.*

- **Section 3.2**. There are two aspects of the test. First, a diagnostic is introduced to expose fibrillation's caused by instabilities. This is sufficiently explained. However, the test has an additional aspect: to forecast modifying the parametrization time-step only while the dynamics timestep remains unchanged. In my understanding, at each model step the tested parametrization is integrated using a single (no sub-stepping) half timestep without reaching $t+\Delta t$ but terminating at $t + \Delta t/2$. If this is true then the integration is incomplete. Furthermore, I am not convinced that this is a good test for stiffness for the reason that stiffness is more likely to be exposed when time steps are increased and not when decreased. I think that what the author wishes to achieve is to introduce a large truncation error and see how this is propagated in time. More reasons have to be given why is this a good test for stiffness. My thoughts about this:

  - The rapidly decaying nature of a stiff problem implies that, for an unconditionally stable numerical scheme with strong damping at infinity, any perturbation is quickly damped. This implies that if a good stable scheme is used the above test may not produce any oscillations but its effect may just be a drift in the solution as already mentioned by the author. In other words stiff symptoms may not be exposed. I think that a lot can be learned by using the simple problem in section 2. To compare the noise when scheme (2) is propagated with timestep $\Delta t/2$ but the slow part of the solution $D(t)$ is correctly updated at $t + \Delta t$, i.e. the error is in the fast part. If this is already happening then please make it more obvious in the text. However, my impression is that this may produce less noise in some cases.

- **Abstract and section 4**. It is claimed that "The test has identified the stratiform precipitation scheme (a diagnostic Kessler type scheme) as a stiff problem, particularly the term that describes evaporation of snow". My objection to this statement is that stiffness is a property of a system of differential equations and therefore we cannot label a diagnostic scheme as a stiff problem. What I think happens is that due to the coupling of precipitation/evaporation processes with the surface and the vertical diffusion scheme the modified settings of section 4 have an impact on these which are the true stiff problems. I suggest to the author to consider this and to try to give a more precise statement on the observed behaviour.

3 Technical corrections

- **Abstract**
  - 2nd sentence: I think it is sufficient to say ""Semi-Lagrangian advection schemes allow long time-steps."
  - Sentence starting "But small oscillations remain in an operational ..." remove **"an"** from there. It is better to delete the next sentence and merge the following two to something like: "In this paper a simple test is designed to reveal if the formulation of a particular physical ..."

- **Introduction.**
  - Second sentence before the end of 1st paragraph: is it perhaps more appropriate to say " resulting in **occasional** instabilities in operational ...."? Or these long time steps were indeed causing frequent model failures?
  - The caption in Figs 2,3 has a mistake: the plots show numerical solution of Eq (1) (using scheme described by Eq (2)) and not a numerical solution of Eq (2). Furthermore, these figures are too small.
  - The value of diffusion coefficient K in 2.1 needs to be specified.