Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2020-340-RC2, 2020

Geoscientific Model Development
Discussions

Interactive comment on "LISFLOOD-FP 8.0: the new discontinuous Galerkin shallow water solver for multi-core CPUs and GPUs" *by* James Shaw et al.

Anonymous Referee #2

Received and published: 22 December 2020

Review of "LISFLOOD-FP 8.0: the new discontinuous Galerkin shallow water solver for multi-core CPUs and GPUs" By Shaw et al.

Major remarks

The inclusion of a second-order DG2 discretisation and implementation in the LISFLOOD-FP 8.0 flood forecasting suite, on a regular mesh, of the shallow water equations with friction and rainfall, for flood forecasting, is presented, including analyses of three test cases and comparison with a local inertia solver ACC and a first-order FV1 discretisation and implementation. Parallelisation and GPU results are intercompared in terms of performance and runtime.

While the paper is very interesting and the work constitutes a useful addition in a flood forecasting suite for public use, which is very important, there seem to be several loose ends and unclear aspects that need to be resolved before publication is warranted. I therefore recommend to return the manuscript for major revisions.

It would be desirable that the following major issues are addressed, either clarified or resolved:

(i) Since ACC, FV1 and DG2 schemes are tested and compared more clarity on their characteristics is desired in order to understand the differences in the results presented. Why is DG2 much slower than FV1 in the third test? The time stepping schemes of FV1 and DG2 are unclear. Figuring out the split scheme for DG2 given rainfall and damping is too hard or impossible also, since the references are unclear; it is easier to simply state the full scheme; there is sufficient space in (9) to do so and remove any ambiguity. The split scheme for Sf is not implicit, once one looks into the references. Why is the handling of friction (Sf) in DG2 leading to troubles for thin, fast overland flow while it is not troubling for FV1 and ACC? Perhaps use the same time stepping scheme in DG2 as for FV1 or make aspects of Sf semi-implicit. Please clarify and make improvements.

(ii) In test 3, but also in earlier tests, DG2 has topography at dx=40 (or another dx for tests 1 and 2) and seemingly the higher degrees of freedom used for the variables is not used for the topography, i.e. the dx=10 topography information can be used to make finer projections onto the topography given that DG2 is second order and the topography should not be planar. Hence, the (at times and in certain simulations) observed simulation underperformance of DG2 seems to be/is more severe than needed.

(iii) DG2 is used on a regular mesh; DG is most optimal for non-uniform situations but this option or restriction is not discussed; also not discussed are hybrid 1D and 2D schemes or hybrid FV1 and DG2 options, the latter which should be easy to accommodate. At least a discussion is warranted.

(iv) Some of the speed and/or accuracy comparisons seem incomplete or unfair: e.g., in Fig. 6, not the same resolution should (only) be compared but (also) the speed for mixed resolutions with (roughly) the same accuracy.

(v) The is a large accumulation of minor remarks, see below, which should be refuted an addressed.

The above major remarks are also reflected, sometimes on several occasions, in several of the minor/detailed remarks given below.

Minor/detailed remarks

Line 23: simplify (plural)

Line 42: But there are plenty of FV2 solvers, which may be faster; has a comparison with those been made or at least discussed? Please discuss.

Line 56: What about mixed 1D and 2D solvers, see the literature?

Line 58: This paper [use of "remainder" is a bit much on page 2].

Line 72: What is new or simplified relative to K2018, and why?âĂÍ Please specify

Eqn (2): define x, y, t.

Line 80: define L and T.

Line 83: I don't like calling Cf a coefficient since it includes a dynamic variable, i.e. h; rephrase; it is a friction function. I would take out hˆ{1/3} (of Cf) and even write it is hˆalpha since alpha is a parameterization anyway.

Line 90: Why is this a dot product? No dot; it is a matrix-vector product.

Line 119: there should be a flag doing that (application of a slope limiter) automatically for smooth solutions, not a hand-switch. See Krivodonova or extensions for improvements. In any real flooding case, this should be done via an automatic switch. Please clarify and add a sensible automatised switch. That the test cases do not involve

shock wave propagation, is that not an omission? Tests 2 and 3 must have seen some shock-wave propagation prior to settling into a quasi-steady state, given the F>1, F<1 transition observed and discussed. Please clarify.

Line 121: In KS2020, I do not see thus split implicit scheme explained? Rather KS2020 refers to KL2010, where matters are also not explained, so reference is inappropriate. LM2009 do show a split scheme but it is not really implicit. I find (8) to (9) vague. Too convoluted. Simply give the entire scheme in 9 (there is ample space) then we all know what is done rather then having to piece somehow together what is factually done (which I failed to do). Comment whether the schemes for Sf and rain are 2nd or 1st order also per the comment on eqn (8) below. Given that Sf is kind of quadratic in u (or hu) a semi-implicit scheme is almost instantly made up. That would be straightforward for (9a) and even easy for (9b). One can check the formal time accuracy for the case with only Sf and R on the RHS. And perhaps even for a constant bottom slope river kinematic limit (which is an exact limit within the SWE).

Line 121b: Why is DG2 slower; due to the CFL=1/3 criterion relative to CFL $\sim$0.9 for FV1?âĂÍ Eqn (8): why is this scheme for rainfall 2nd order? Why is rain not directly included into (9). R=R(t) so non-autonomous RK would be fine? Please explain your choices better. Please provide the entire scheme for the entire system in (9); there is space and currently the time stepping scheme is unclear. Please clarify.

Figure 2: How can one apply rainfall if dt is not know yet, since dt is determined later in the diagram? Order seems off?

Line 157: typo; L missing (only subscript y seen).

Line 173: add OUP "and," at end of third option.

Line 183: Can it be clarified earlier whether or not (I think it is "or not") DG2 can be truly Cˆ0? Since the x*y term is not include, therefor it cannot represent Cˆ0 solutions or topography. While speed is gained this drops formal Cˆ0 smoothness. Say so explicitly, probably earlier, also why this option with only polynomials 1, x and y is chosen, I assume to enhance computational speed.

Line 186: Nothing is said about the time stepper for FV1 and ACC? Why does FV1 do better for thin overland flows? How are Sf and R dealt with in FV1? This information is relevant to understand the test results provided and analysed later. Please clarify (earlier on).

Eqn (14): Perhaps add some spacing around \Delta t.

Line 207: Second-order wrt to what since it cannot be 2nd order wrt solutions requiring the advective velocity terms? Please define clearly what is meant here? When advection is important, it cannot be second order, of course?

Fig. 6: Since FV1 is order one that use of the same grid spacing in the different models is a priori unfair. What happens if FV1 is adjusted (and maybe ACC) such that the expected errors are the same? Please add that situation. The left bottom panel could be done for finer resolution as well, as in Fig 7.

Fig. 7: Figure a bit large. Otherwise nice.

Line 270 paragraph: But this comparison is unfair; one needs to compare run times for the same or at least similar accuracy; either focussing on overall accuracy or the accuracy at the front since the latter may be most relevant. This needs an extension.

Line 283: Statement by authors themselves underscores my previous remark. So why is this case not made in the paper with the same convergence/accuracy compared with runtime?

Fig. 8b,c: Same remark; what is the right comparison here? Please address. Caption: page number and caption overlap.

Fig. 12: Can you show simulations for DG2 with dx=50m, say, as well since that runs in less time than the other models but the solution may still be better?

Table 1: So DG2 dx=50m should be compared with runtimes of the other models or even DG2-GPU at dx=25m should be compared with the other models, in line with the previous five remarks.

Line 352: Why is the domain initially dry? How long does it take for the memory of this odd initial condition to disappear?

Line 356: I do not understand this remark “No Data”; translate this computer remark.

Lin 364: But that seems unfair. Finer data need to be projected on the topography that DG2 can model, which is not dx=40m but the finer one projected? In fact, DG2 would then do a bit better I suspect. Address this please; perhaps really redo the DG2 case; it should have 3 dofs per cell, also for the topography.

Line 367: Why is repositioning required since across the river channel the river level is within 0.2m, say, the same. Is the level meant the river level height above sea level? What is recorded? So why is this repositioning needed, since variation across the river channel will be minimal. It is free-surface height that matters. Please clarify.

Fig. 14: I am not convinced by the DG2 results due to not taking into account slope information in the bathymetry. So DG2 may be underperforming while this can partially be repaired. Please update and clarify.

Line 408: As stated above this anomaly of using only a planar representation in DG2 should be fixed: you do not use DG2 optimally with only 40m planar resolution on bathymetry, while you are allowed to include higher resolution and project on the DG2 basis you use. This leads to an unfair comparison which seems not aligned with the DG2 capabilities.

Line 418: What is the error in the observations? What is the variation in surface level across a (flooded) river channel? What is consequently the result of shifting the measurement position? I would guess the error is circa 0.2m (from watching river levels bob about +-0.1m for a river in flood next to a river gauge). What is the variation within

10m along stream of the surface level in the simulation and across the channel, in order to get an error estimate? Please clarify and add a discussion on errors, in both the observations and simulations.

Line 425: Please add the word “maximum” before the last word “flood”.

Line 428: “More notable differences” Where? Clarify please, e.g. by using arrows.

Line 428: On flood defence walls not being captured on coarse grids: unless, of course, you put those in by hand, which should be done (regardless). Here is where variable/non-uniform mesh capabilities of DG2 or FV1 would have come in handy. These are then essentially (vertical) walls with a finite vertical extent. Also on a coarse mesh one can add vertical walls at cell edges as approximation.

Line 432: So, what then is right or wrong, ACC or FV1 and how do we know that? Please clarify.

Fig. 16: Graphs in Fig. 16 look all the same; use arrows to highlight areas you want the reader to focus on.

Fig. 17: I still disagree with a DG0 bottom topography at dx=40 for DG2. That is a false comparison, cf. previous remarks made. Further: what flood accuracy do we actually care about and what are the error bars given errors in rainfall? Please discuss.

Line 491: But as said before, your topography is incorrect, in not matching DG2 accuracy that can be used. Also, I am not convinced by your time stepping in DG2 as compared to what is done in ACC and FV1 (the latter which is unclear as well and makes it difficult to understand the comparison). Please clarify.

Line 501: The conclusion here is a bit of a downer for DG2; no discussion is made on using hybrid approaches, including variables time and spatial resolution for which DG should be good and more promising. Please comment and update.

Appendix B: I am a bit confused; why do we need to linearise to establish whether ACC

is second order? Please clarify.

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