1 Review

The manuscript *r.avaflow v1, an advanced open source computational framework for the propagation and interaction of two-phase mass flows* describes a GRASS integrated software framework for the simulation of gravity-driven mass movements. Its underlying mathematical model and corresponding numerical solution are content of the author's previous work. In this manuscript they focus on details regarding the integration of the numerical solution algorithm with a GRASS GIS environment, as well as additional functionalities that are needed when wanting to validate the simulation model with data. Performance and software framework are demonstrated during two test cases, one being different scenarios of a complex landslide in a synthesized topography, the other being the re-analysis of a New Zealand rock avalanche. The manuscript concludes with a discussion of necessary next steps and an outloook.

2 General impression

Generally, the paper is well written and easily comprehensible. It addresses the need for software frameworks that can be used to validate state-of-the-art mathematical models against field observations. Though r.avaflow isn't fully validated and ready to use in a predictive regime, the general approach and software solutions presented by the authors are a valuable contribution to the community. I do have three major comments and some minor objections, which are listed in the following.

3 Major comments

1. You omit including a description of the mathematical model, and rather refer to a publication (Pudasaini 2012). I agree to the first reviewer that this in principle is acceptable. However, at several points in your manuscript you mention that you work with an 'enhanced' version of Pudasaini 2012. It is not at all clear to the reader what these 'enhancements' are! Is it the 'complementary functions' detailed in 2.4.? Please give more details how you deviate from Pudasaini 2012.

2. To me, the 'complementary functions' block seems to be composed of three groups:
   ID 1,2 and 3 compensate for deficiencies of the numerical scheme. These shouldn't be necessary, if the scheme was shock-resolving, volume and positivity preserving, and well-balanced. You touch upon this in your outlook, when you state that you want to work on the solver in the future. Valuable to the current reader would be a summary of the properties (pro and con) of the current numerical scheme, and if possible, an order of magnitude of the modifications introduced by the 'complementary functions' ID 1-3.
   ID 4: It is straight forward to consider entrainment in the model equation (as you also mention in your manuscript). It seems inconsistent not to have this as a part of the numerical scheme. Could you comment on why you chose this approach? And again: is there any additional error expected? Are the update time steps the same for the numerical solver and the complementary function?
   ID 5: I like the idea to derive a proxy for the local run-out length. The idea seems closely related to the very common macro-scale approach that relates fall height H to horizontal runout length R (basically stating that the potential energy has to equilibriate work done by friction):

$MgH = \mu gM \int_A^B \cos\zeta dl = \mu gM \int_{x_A}^{x_B} dx = \mu gM(x_B - x_A)$
A and B being locations on the topography, and x_A and x_B their projections on a flat plane with $R = x_B - x_A$. This boils down to $R = H/\mu$ for a block of material that initially has been at rest.
Though your extension to account for initial kinetic energy is straight forward, I am confused by the fact that equation (6) states that material of initial velocity $v_0 = 0$ will have an $s_{stop} = 0$ (regardless of local inclination and friction coefficient). The situation that you sketch in Figure 3(b) for instance implies a certain s_{stop}. If the slope at which the material comes to rest is steeper that the basal friction angle, however, it should start to flow again in counter direction. How do you account for that based on your stopping criteria?

3. Is it possible at all to use r.avaflow with a self-written numerical solver? It would be of very high value if the model/solver could be easily substituted. Can you describe the interface between the numerical method and the GIS framework in more detail?

4 Minor objections

- Abstract and introduction
  - Please explain what you mean with 'more or less complex process chains and interactions'. My impression is that you mean subsequent events, or events of delayed release time and potentially varying initial conditions. Is that right?
  - The more recent work of Iverson (Iverson and George, George and Iverson 2015) provides another two-phase approach for mass flows and would be good to mention. The same is true for GeoClaw which is the corresponding software tool.
  - In the introduction you say that none of the models includes the possibility for computing cascaded events. This is actually not quite right. For example in RAMMS you also have the functionality to add deposit onto the topography to study subsequent overflow.
- The computational framework r.avaflow
  - What is the difference between EXPERT and PROFESSIONAL?
  - Figure 1: Change of basal topography (s,f,b) is this a typo, or why '(s,f,b)'?
  - Please describe your understanding of a 'pixel' and its relation to the surface mesh on which the system is solved. Is the 'pixel' equivalent to a surface grid cell? Or is it rather the projected grid cell?
  - equidistant quadratic cells: this is a structured, regular grid then?
- Computational experiments and discussion
  - What criteria / objective functions has been used to decide for the 'optimal parameters'?
  - Figure 11: Is any of these the aforementioned objective function? It is hard to interpret the plots. Can you re-scale the plots to better see the optima?

Best, Julia