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Geoscientific Model Development Discussions

Interactive comment on "faSavageHutterFOAM 1.0: Depth-integrated simulation of dense snow avalanches on natural terrain with OpenFOAM" *by* Matthias Rauter et al.

Anonymous Referee #1

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This paper describes a numerical implementation of a snow avalanche model on complex natural topography within the OpenFoam open source toolkit. The topography is written in terms of surface partial differential equations, which makes extension to complex terrain straightforward within OpenFoam. This seems a nice way of programming the topographic effects. An example calculation is shown for the Wolfsgruben avalanche. The authors focus on an implementation of the Savage & Hutter (1989) model that has been generalized to 2D and contains many non-standard features, that the authors have programmed up, but I wonder how well they actually model the real physical system? There is a danger here that by providing an easily useable code it will receive widespread use by others and that the physical description gets engrained

in the community without adequate testing or questioning of the physics in the model.

1/. Equation 1 introduces a mass source through basal entrainment, but there seems to be no corresponding momentum source/sink in equation 2, is this correct? Surely there is some momentum exchange with the base, and it might be different depending on whether the avalanche is eroding or depositing material.

2/. Field studies of [SOVILLA, B., BURLANDO, P. & BARTELT, P. 2006 Field experiments and numerical modeling of mass entrainment in snow avalanches. J. Geophys. Res. 111, F03007; SOVILLA, B., SOMMAVILLA, F. & TOMASELLI, A. 2001 Measurements of mass balance in dense snow avalanche events. Ann. Glaciol. 32, 230–236] seem to suggest that most of the entrainment occurs by frontal ploughing rather than basal erosion. In fact some of the earliest snow avalanche models [BRIUKHANOV, A. V., GRIGORIAN, S. S., MIAGKOV, S. M., PLAM, M. Y., SHUROVA, I. E., EGLIT, M. E. & YAKIMOV, Y. L. 1967 On some new approaches to the dynamics of snow avalanches. In Physics of Snow and Ice, Proceedings of the International Conference on Low Temperature Science, vol. 1, pp. 1221–1241. Institute of Low Temperature Science, Hokkaido University] modelled the entrainment between a shallow water-like avalanche and a static layer of snow in front of it by using jump conditions across the interface to couple the two domains. Isn't this a better approach?

3/. I think the only way of really testing whether the entrainment model is any good is to compare it to carefully controlled laboratory experiments where the amount of entrainable material is known and there are key morphological features than can be seen in the subsequent deposits [EDWARDS, A. N., VIROULET, S., KOKELAAR, B. P. & GRAY, J. M. N. T. 2017 Formation of levees, troughs and elevated channels by avalanches on erodible slopes. J. Fluid Mech. 823, 278–315.] I strongly suspect that the current model will not be able to capture any of these features. It should be noted that very similar deposit features are often seen in snow avalanches at the Vallee de al Sionne, as well as at many other sites such as the Geschinen avalanche [ANCEY C. Are there "dragon-kings" events (i.e. genuine outliers) among extreme avalanches?

Eur. Phys. J. Special Topics 205, 117-129].

4/. One of the interesting features of erodible layers is that they appear to give avalanches far greater mobility (MANGENEY, A., ROCHE, O., HUNGR, O., MANGOLD, N., FACCANONI, G. & LUCAS, A. 2010 Erosion and mobility in granular collapse over sloping beds. J. Geophys. Res. 115, F03040), so there is a direct link to the apparent basal friction that the avalanche experiences. At the moment the drag law in equation (9) is completely decoupled from the erosion, so how can this be right?

5/. I also wonder how the basal friction law is able to keep material in the run-out zone stationary. As the velocity tends to zero, there is no basal friction, so I would expect the snow deposit to creep. The only way I can see things staying static is that everything has been deposited, i.e. the avalanche thickness is zero, can the authors clarify? This would actually speak in favour of having basal deposition as well as frontal ploughing.

6/. Coming back to the momentum balance (2) I note that the earth pressure coefficient has been neglected in the depth-integrated pressure gradient term. This was one of the key features that distinguished the Savage & Hutter (1989) theory from earlier Russian models. Is faSavageHutterFOAM 1.0: then a good name for the code? One of the problems of having the earth pressure coefficients is that they introduce stress discontinuities which generate shocks in the height and velocity of the flow. However, numerous observations [e.g. GRAY, J. M. N. T., TAI, Y. C. & NOELLE, S. 2003 Shock waves, dead-zones and particle-free regions in rapid granular free-surface flows. J. Fluid Mech. 491, 161–181; FAUG, T., CHILDS, P., WYBURN, E. & EINAV, I. 2015 Standing jumps in shallow granular flows down smooth inclines. Phys. Fluids 27, 073304.] demonstrate that only shocks that are expected from shallow-water/hydraulic type avalanche models are observed. Certainly there is no evidence for stress shocks from switching discontinuously from active and passive earth pressure regimes.

7/. There is no vertical acceleration in equation (3). Is there a good reason for that?

8/. In equation (4) the integrand is u(x_b-n_b,z'). I don't understand why we need the

-n_b term. Why not just u(x,b,z')?

9/. At the bottom of the page the definition is made that $uv=u\otimes v$ where $\otimes$ is the dyadic product. I think this contraction in notation is not good - I would recommend retaining the dyadic product $\otimes$ in the paper. This is because at the moment matrix-vector multiplication is now shown with a dot product in equations (5-8). Just keep the standard notation.

10/. Are equation (5)-(8) right? I looked at the Deckelnick et al (2005) reference and found the equations the authors mention hard to find. Just working out the tangential and normal components of the gravity vector g in a coordinate system aligned with the slope (such as that used by Savage & Hutter 1989) seems to give me some results that I find hard to interpret. It seems this gives a slope normal and tangential representation that is then translated back into a Cartesian coordinate system aligned with gravity. If I'm struggling here, then I suspect others will also.

11/. Isn't a consequence of the friction law (9) that one gets steady states in which u∼sqrt(h)? This seems to conflict with small experiments that show u∼hˆ(3/2) [POULIQUEN, O. & FORTERRE, Y. 2002 Friction law for dense granular flows: application to the motion of a mass down a rough inclined plane. J. Fluid Mech. 453, 133–151]. Is there any evidence for this scaling?

12/. page 6, lines 10-15: "First-order schemes converge slower in terms of mesh refinement due to their high numerical diffusivity. However, numerical diffusivity effectively prevents oscillations and increases the stability of the solver. Oscillations in second-order accurate simulations are prevented with a normalised variable diagram (NVD) scheme for unstructured meshes, known as Gamma scheme (Jasak et al., 1999)." I wonder whether the oscillations that are being prevented are purely numerical? There can also be physical oscillations that are related to roll-waves which are observed in avalanches at a range of scale [FORTERRE, Y. & POULIQUEN, O. 2003 Long-surface-wave instability dense granular flows. J. Fluid Mech. 486, 21–50.; Kohler, A et al 2016

The dynamics of surges in the 3 February 2015 avalanches in Vallee de la Sionne JOURNAL OF GEOPHYSICAL RESEARCH-EARTH SURFACE Volume: 121 Issue: 11 Pages: 2192-2210; RAZIS et al. 2014 Arrested coarsening of granular roll waves. Phys. Fluids 26, 123305.] Are these being suppressed artificially by the numerics? Or does the friction law (9) not generate roll-waves?

13/. There are no scales in figures 3-6 or indication of the change in topographic height. The authors are probably so familiar with the simulations that this is obvious to them, but it is not obvious to the reader.

14/. page 12 line 11: modulus signs are missing on |\bar u|< ...

15/. Figures 7-9: I know making comparisons to deposits from natural events is widespread in the literature, but can one really learn very much from this type of simulation and comparison. There seems so much physics that we don't understand properly and so much uncertainty about the initial conditions, snow entrainment and feedback on friction, that it is not surprising that we can't model the deposits accurately. Furthermore the friction coefficients can always be adjusted to get us in the right ball park for the run-out. Isn't there a more fundamental comparison that can be made?

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