Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2018-67-AC1, 2018

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.

Matthias Rauter et al.

matthias.rauter@uibk.ac.at

Received and published: 25 April 2018

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.

Dear Referee,

thank you very much for your fast review and your interesting remarks on the manuscript. We share many of your concerns regarding the accuracy of the physical model and the validity of friction and entrainment relations. However, the applied flow and process models present a setup that is currently widely used, as shown in a recent survey [Schmidtner, Korbinian; Sailer, Rudolf; Bartelt, Perry; Fellin, Wolfgang; Fischer, Jan-Thomas; Granig, Matthias (2017): Investigations on the Application of Avalanche Simulations: A Survey Conducted among Avalanche Experts. In: ICNDAHR 2017: International Conference on Natural Disasters, Assessing Hazards and Risk, London, United Kingdom, (May 25-26, 2017). Canakkale: World Academy of Science, Engineering and Technology (WASET)]. We stick to the "standard setup" to not get lost in the discussion about appropriate closure relations and to focus on the general framework of faSavageHutterFOAM. Note that various alternative friction and entrainment models can be selected by the user (See page 5, lines 17ff). This includes friction models fulfilling Bagnold scaling ($\overline{u} \sim h^{(3/2)}$) and a front entrainment model, as you propose in your comments. We refer to the manual within supplementary materials for details. Moreover, the open structure allows simple application of custom models. This is, in our opinion, especially important, allowing everybody to apply the preferred friction and entrainment models. Finally, we want to note that developing and evaluating more realistic process models requires a platform for testing, which the proposed solver is intended to provide.

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.

Entrained mass has no velocity at the time of erosion. The respective momentum is therefore zero and no momentum source has to be taken into account. Note that we use the conservative description. In a non-conservative formulation, an entrainment term will show up in the momentum equation. Momentum exchange with the base is considered with the basal friction term τ_b. The interaction between entrainment and rheology is usually not considered for practical applications, although considered important by many scientists. We are aware of such approaches and they can be easily implemented in faSavageHutterFOAM.

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?

Such models can be implemented. See answer below.

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].

We agree that the shown model may be to simple to model such effects. More elaborated models can be implemented in the code (some are already available). However, it is out of the scope of this paper to compare different models. This will be undertaken in future research.

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?

The evaluation of most friction and entrainment models is still missing. We stick in this paper to the simplest and commonly applied relations as the main goal was to show the basic performance of the new implementation method in comparison to well established ones. Various entrainment models (including front entrainment) are available in the presented code and can be selected in transportProperties. Moreover, the framework is designed to simplify the implementation of new entrainment and friction models. See manual within supplementary materials for details. This will hopefully help to select the appropriate physical models in the near future.

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.

You are correct. However, the creeping velocity is approximately $10^{-5}\mathrm{m/s}$. This value is lower than the tolerance of the solver and thus virtually zero, especially when compared to the creeping velocity of other implementations. We will add the respective remark to the revised manuscript.

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.

We are aware of the fact that contributions to depth-integrated avalanche modelling are highly controversial. We try to accurately describe the historical developments in the introduction. We try to attribute accomplishments to the appropriate groups to the best of our ability and the Russian models are mentioned as well.

We think that the work of Savage and Hutter (1989) has been one of the most influential ones, introducing depth-integrated avalanche simulations to a wide audience. The name Savage-Hutter model has become a synonym for depth-integrated avalanche models, which is also acknowledged by competing scientists, e.g. [Salm, Bruno. "A short and personal history of snow avalanche dynamics." Cold Regions Science and Technology 39.2-3 (2004): 83-92.]: *"Here I would like to pause and emphasize that as early as 1966, the foundation of what is now known as the Savage–Hutter theory (Savage and Hutter, 1989) was squarely in place."*

The decisive difference between the Savage-Hutter-model and other depth-integrated models in OpenFOAM is the assumed direction of the velocity (horizontal vs. surface tangetial). We use the respective name to distinguish our model from strictly two-dimensional ones with horizontal velocity (e.g. ShallowFOAM, https://github.com/mintgen/shallowFoam).

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

The surface normal acceleration of the flow is zero, according to the model assumption of surface tangential velocity, $(\mathbf{n}_\mathrm{b}\,\mathbf{n}_\mathrm{b})\cdot\overline{\mathbf{u}} = \mathbf{0}$, see Rauter and Tukovic (2018). Thus no acceleration is present in Eq. (3). The vertical acceleration is included in Eq. (2) as part of the surface tangential acceleration, $\frac{\partial}{\partial t}\left(h\,\overline{\mathbf{u}}\right) = \frac{\partial}{\partial t}\left(h\,(\overline{u}_x,\overline{u}_y,\overline{u}_z)^\mathsf{T}\right)$.

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 $u(x, b, z')$?

The function $\mathbf{u}(\mathbf{x})$ maps global Cartesian coordinates $\mathbf{x}$ to the velocity in the same coordinate system. The notation including the surface normal vector $\mathbf{n}_\mathrm{b}$ was proposed by a referee of Rauter and Tukovic (2018). The surface normal vector should indicate the direction of depth-integration, which is surface normal.

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.

The current notation has been adopted from common literature on the subject (e.g. Craster and Matar, 2009; Tukovic and Jasak, 2012; Rauter and Tukovic 2018) and we would like to be consistent with them.

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.

Equations (5) and (6) are simple projections, which are presented for a didac-

tic reason. They introduce the projection of vectorial entities on a surface. This is a common procedure in CFD [e.g., Vukcevic, V. Numerical modelling of coupled potential and viscous flow for marine applications. PhD thesis, Faculty of Mechanical Engineering and Naval Architecture, University of Zagreb, 2016., https://dx.doi.org/10.13140/RG.2.2.23080.57605, page 16] and showing the example with the gravitational acceleration makes the concept easier to grasp. Equation (7) can be found in Deckelnick et al (2005), section 2.1 (note that variable names are different) and Eq. (8) is an extension, which has been introduced by Rauter and Tukovic (2018).

No surface aligned coordinate system is required to work out the projects. In fact, thinking in surface aligned coordinates is not suitable to understand the presented method.

We propose to add a simple introduction to the applied projections to the appendix of the manuscript (see appended document).

11/. Isn't a consequence of the friction law (9) that one gets steady states in which $u \sim sqrt(h)$? This seems to conflict with small experiments that show $u \sim h^{(3/2)}$ [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?

The applied friction law does not obey Bagnold scaling ($\overline{u} \sim h^{(3/2)}$), that is correct. We are aware of this issue and used other friction laws, which obey Bagnold scaling, in previous works (see Rauter et al., 2016; Rauter and Tukovic, 2018). As mentioned in the manuscript (page 5, line 17ff), we chose the Voellmy friction relation to resemble the traditional model. Various friction laws are available in the code and can be selected by the user (see manual within supplementary materials for details). This includes various friction laws which obey Bagnold 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?

The application of the discussed schemes is imperative for the solution of hyperbolic partial differential equations and generally accepted in CFD. Schemes are designed to solely suppress numerical oscillations, although this is difficult and not always possible. The applied friction law does not generate roll-waves. Therefore, oscillations have to be purely numerical (see, e.g. Wang et al., 2004, for a demonstration). The solver should be able to generate roll-waves with other friction laws.

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.

That is a valid point. However, in Figure 3 (x,y,z)-coordinates in the co-ordinate reference system "MGI/ Austria GK West" (EPSG: 31254) (see: http://spatialreference.org/ref/epsg/31254/) are provided for each subfigure - the z coordinate gives an indication for the change in topographic height for the calculation domain from release area to the valley bottom. Scales for horizontal distances are provided in Figures 3,6,7,8 and 9. In the revised version we will add labels to the contour lines in figure 7 for clarification of topographic height changes.

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

This is correct. We will add modulus signs in the revised manuscript.

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?

We are aware of the manifold ways to compare simulations. However, the main intention of the paper is to present the new solver and to show that existing results (of e.g. SamosAT) can be roughly reproduced.

Comparisons to analytical solutions can be found in Rauter and Tukovic (2018).

Moveover, we conducted simulations of avalanches from test sites Ryggfonn and Valleé de la Sionne, including comparisons to radar measurments [Rauter, M. (2017):

A finite area scheme for shall ow granular flows on three-dimensional surfaces. Poster presented at: European Geosciences Union (EGU) General Assembly 2017, Wien, 24.04.2017; Rauter, M., Köhler, A. (2017): A Finite Area Scheme for Shallow Granular Flows. Presented at: 12th Workshop on OpenFOAM (OFW12), Exeter, 26.07.2017]. However, methods for comparison are complex and would distract from the primary goal of the paper. Moreover, the underlying data is not freely available and therefore, not siuted for GMD, which highly values transparency.