Response to reviewer comment 2 (RC2) by Stefan Hergarten

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We wish to thank Professor Stefan Hergarten for his constructive comments. We reproduce below the review comments in italics, followed by our reply to each comment in normal type. The possible corresponding changes made to the manuscript are reproduced in blue.

Referee: 2

Review Comment:

In this paper, a theoretical and numerical model for the morphology of the deposits of debris flows is presented. As a main simplification compared to existing models, effects of inertia are neglected. While existing models are based on shallow-water type (Savage-Hutter) equations, this approach arrives at a nonlinear diffusion equation with a threshold slope. For validation, analytical solutions, topographies of real debris flow deposits, and laboratory experiments (being a part of the study) are used.

First, I would like to emphasize that both the theory and the numerical implementation are described very well and in great detail. Since the diffusion equation is numerically not very challenging, one might even ask whether such a detailed and basic level is necessary. However, I do not complain about this.

Our reply:

We thank Professor Hergarten for appreciating the manuscript, and describe below the changes made to address the major and minor comments.

Review Comment:

My main criticism concerns the simplification by neglecting effects of inertia. As stated by the authors, this limits the applicability to low velocities. The question whether this is a serious limitation for the application to real debris flow is not addressed sufficiently. All results used for validation are solely based on the final final topography and thus on the very end of the movement when the velocities should indeed be small. On the other hand, the introduction starts from the hazard of debris flow, where the runout length is more important than the morphology of the deposits. So the authors should point out more clearly that the referenced existing models also attempt to predict the runout also at high velocities, while this is not tested for the new model. It even looks as if the new model mainly constructs a final deposit topography that obeys a predefined relation between slope *and thickness.*

Our reply:

Thank you for raising the concerns. This is indeed what we do. To address this point, we propose to add the following clarification to the introduction:

Unlike existing models that also attempt to predict the runout at high velocities, we limit our scope and focus on predicting the final deposit morphologies of debris flows, modelled as slow, quasi-static processes.

Review Comment:

As a second point concerning neglecting effects of friction, I am not fully convinced that it makes things simpler or more efficient. It is stated that the existing models require a large amount of input data. However, can go back to the original Savage-Hutter equations with a simple static friction term and nothing else. Then the coefficient of friction would be the only model parameter. We could also go a step further and use the Mohr-Coulomb criterion as proposed in the recent manuscript. The number of parameters and their meaning would be almost the same in both models then. This scenario would allow for an assessment of how much we lose by neglecting effects of inertia and how much we save. Theoretically, we save much because the equations become simpler. However, the results about the computational performance given in Table 1 are disappointing. It seems that the diffusion model model with the explicit time step requires very small time increments. Without having data for comparison available, it looks to me as if the new model was quite inefficient compared to existing models.

To summarize these points, it would be essential for me to see a thorough analysis of what we lose with regard to real debris flow with the new model and whether there is any increase in numerical efficiency.

Our reply:

As pointed out by the Reviewer, the numerical model as described in the previous version of the manuscript was not computationally very efficient. To improve on this point, we have now modified our algorithm so that dynamic time steps can be used instead of constant time steps. By doing so, the model efficiency is now greatly improved, reducing the computational times by more than an order of magnitude (see updated Table 1 below). Further, we see a similar speedup in the simulations of experiment cases.

Aside from computation time, another key consideration is the work involved in calibrating model parameters. In this regard, an important advantage of our proposed simple model is that its parameters can be calibrated directly from topography profile data. As done in the paper for the experimental cases, all model parameters can be acquired from a single long profile through observed deposits. It is therefore not necessary to run the three-dimensional model multiple times to adjust model parameters by trial and error. More complex models, by contrast, typically require multiple iterations, or must rely on other sampling and material analysis to acquire parameter values.

Table 1. Influence of mesh size on model accuracy and computational time.

Avg. element size [m]	# of elements	$(h-H)/h$	$(R_{10\max}-R_{10\min})/r_{10}$	Computational time [s]
0.265	526	0.063	0.092	0.092
0.132	2116	0.032	0.037	2.46
0.066	8612	0.020	0.012	46.5
0.033	33986	0.011	0.007	1117.4

Review Comment:

Provided that this can be done, I would also suggest to consider the following aspects:

Section 2: If I got it correctly, the flux is only dependent on the slope, but not on the thickness (above a minimum thickness). This means that the flow velocity increases with decreasing thickness. I would have rather expected a flow velocity that depends on the slope only. I guess that the rather high fluxes at low thickness arising from the approach used here are not very good for the numerical performance. Is there a specific reason for this approach?

Our reply:

We thank Professor Hergarten for this interesting suggestion. Indeed when comparing the verification test runs, where the deposit thickness changes systematically from input to toe, modifying our diffusivity in the manner suggested, resulted in an additional 3 fold speed up in calculation time. However in simulating the experiments, where changes in the deposit thickness are not as systematic, there was a 3 fold slow down. So while the idea of exploring how modification of the diffusivity term may influence CPU is worthwhile, based on the results we have, we will retain our current algorithm, with the adjustable time step. As noted above this provides a 10 fold speedup in calculation time from the algorithm used in the original submission.

Review Comment:

Section 3: Rather for curiosity (since I am not an expert on this): Why did you not use a standard Delaunay triangulation in combination with Voronoi polygons as control volumes?

Our reply:

Using Voronoi is an intriguing suggestion. In general, however, the vertices of the Voronoi cells do not coincide with the centers of the Delaunay triangles, and may even lie outside these triangles. By forcing the edges of the control volumes (CV) to have their vertices either at the centers of our triangular elements, or at the midpoints of the triangular sides, better accuracy can be achieved. Moreover, equal weights can be used to interpolate vertex values from node values, which facilitates calculation and bookkeeping.

Review Comment:

Equation 10: How did Qin come in here compared to Eq. 8, and what is it used for? I thought you start the simulation with a given thickness distribution. Or is it just the source term for reproducing the laboratory experiments?

Our reply:

Indeed, the inflow flux Q_{in} is modelled as a source term of limited spatial extent on the right hand side of Eq. (1). To clarify this point, we propose to add this source term explicitly to Eq. (1), to be corrected to:

$$\frac{\partial \tilde{z}}{\partial t} = -\nabla \cdot \mathbf{q} + Q_{in}\delta(\mathbf{x_s}), \tag{1}$$

where δ is the Dirac delta function, $\mathbf{x_s} = (x_s, y_s)$ is the location of the source, and Q_{in} is the inflow source volumetric flux. We propose to also explicitly add Q_{in} to the corresponding numerical statement, and correct Eq. (8) to:

$$\frac{\tilde{z}_i^{\text{new}} - \tilde{z}_i}{\Delta t} = -\frac{1}{A_{CV,i}} \sum_{j=1}^{m} Q_j + \frac{Q_{in,i}}{A_{CV,i}}, \tag{8}$$

By this correction, Eq. (8) can match Eq. (10).

Using these equations, the simulation can start either with a given thickness distribution or with a pre-existing bed and one or more input source(s). For the simulation of each experimental case in Fig. 10 and 11, we started the simulation with a pre-existing bed and assign constant influx through a given simulation time at one or more source(s) points.

Review Comment:

Figure 5: If the deposit thickness H is measured at the apex, I have some difficulties in relating the values to the legend.

Our reply:

Thank you for flagging this error in the legend. We updated the figure with corrected legend, see below.

Review Comment:

Section 7: I am not convinced that the comparison with analytical solutions should be considered so extensively. These comparisons only illustrate that the numerical implementation of the model works and have nothing to do with the applicability of the model. So the excellent agreement should not be stressed too much.

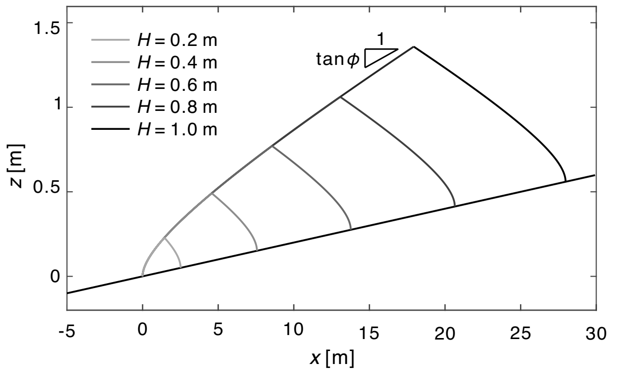

Figure 5. Analytical solutions for the centerline profiles of cohesive-frictional deposits on an inclined plane of slope $\tan\beta = 0.02$, for different deposit heights, assuming identical material properties $\tan\phi = 0.05$, $\tau_Y/(\rho g) = 0.01$ m.

Our reply:

The importance of this comparison is to provide verification of the CVFEM model. However, in line with the review comment, in the revised manuscript we will streamline discussion around this point.

Review Comment:

Anyway, I enjoyed reading the manuscript and like the approach in principle, despite my criticism.

Our reply:

Thank you very much for this final encouraging comment.