Earth Surface Dynamics Discussions

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Comment on esurf-2022-11

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Referee comment on "A control volume finite element model for predicting the morphology of cohesive-frictional debris flow deposits" by Tzu-Yin Chen et al., Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2022-11-RC2, 2022

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.

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.

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.

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?

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?

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?

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

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.

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

Best regards,
Stefan Hergarten