Earth Surface Dynamics Discussions

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Comment on esurf-2022-11

Chris Johnson (Referee)

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-RC1, 2022

This well-written paper combines theory, computational methods, analogue laboratory experiments and field observations of cohesive debris flows.

The paper presents a depth-integrated model for cohesive-frictional debris flows. The model resembles lubrication theory, in that inertia of the fluid is neglected, and the depth-integrated flux is therefore an instantaneous function of the local surface gradient and thickness.

These model equations are solved using a control volume finite-element technique, which is validated by comparison to exact solutions. The model predicts very well the deposits of laboratory experiments of sand/kaolinite/water flows, and is compared with with field observations from Coussot *et al.* (1996).

The agreement between the model predictions and laboratory experiments is striking, and I have no doubt that the modelling approximations made (shallow inertia-free flow with homogeneous cohesive-frictional rheology) are very well suited to the flows in the lab. As a description of laboratory experiments it is therefore a strong piece of work.

However, it is much less clear to me that the physics studied here is relevant to many natural debris flows. There is some acknowledgement of the differences between the modelling here and natural debris flows (lines 52-58 and 441-446). But in my view there should be a much more comprehensive discussion and justification of which predictions from this paper (obtained at laboratory scale) would be expected to hold true at field scale, and which would not. For example:

- It is not clear that yield stress is responsible for the blunt snouts of natural debris flows. In the model used by this paper, the yield stress required to produce blunt snouts at field scale is very large, evidenced by a fit of $\tau_Y/(\rho g) \approx 0.5$m in the field observations, compared to ~0.001m in the experiments. How can this difference of a factor of ~500 in yield stress be explained?
  It seems likely to me that the formation of blunt snouts at field scale could due to a different process, for example the loss of excess pore pressure at the flow front, resulting in a substantial increase in the frictional part of the stress here. That is, the blunt snout could be due to a rheology that is inhomogeneous but not cohesive. Section 8.1 would benefit from some discussion of these points.
- More generally, the difference in physics between small-scale analogue experiments and natural-scale flows has been raised by several authors, including Iverson (e.g. https://doi.org/10.1016/j.geomorph.2015.02.033). The paper would benefit from a discussion of the parameter regime realised in experiments (Froude number, Reynolds number, Savage/inertial number etc.) and a comparison of this with natural examples.
- An important feature of the model is that the inertia of the flow is neglected (line 76). Is this really valid for natural debris flows? Many debris flows are supercritical and exhibit features such as shocks, roll waves and superelevation in curved channels, which require inertia. Is there evidence that inertia can be neglected at field scale?
- It is tempting to attribute the similar conical shapes of the natural debris flow fan (figure 1) and experimental deposits (figure 9) to a similar formation mechanism. Though I do not know the 2009 Xinfa debris flow shown in figure 1, the inundation of houses in this figure suggests a flow of perhaps ~2m deep occurring on a much larger (perhaps ~30m tall?) pre-existing debris-flow fan. If so, this is clearly a completely different mechanism from the *en masse* deposition of the entire fan in the experiments. There is some acknowledgement of this around line 55, but in my view a much clearer statement is needed as to the differences between the modelling/experiments in this paper and the natural deposit in figure 1.

Minor points:

- **Equation (1):** There is no source term in this equation corresponding to the inflow. Is the inflow flux Q_{in} modelled as a source term of limited spatial extent on the right hand side of (1)?
- **Equation (3) / line 83:** I initially misunderstood the statement on line 83 and believed that S_c was a constant for a given material. It may be useful to make it clearer that S_c is dependent on local instantaneous flow depth and free-surface slope, and that this dependence is derived in section 4.
- **Equation (4):** how is this equation derived? From (3), it is clear that the free surface slope is no greater than S_c, but not clear to me why it is exactly equal to S_c. (Derivation of (4) must require some constraints on the initial conditions or inflow functions Q_{in}, as for certain choices of these are counterexamples to (4). For example, if $z_b(x,y) = 0$ and $Q_{in}(x,y) = k$ and the initial conditions are \tilde z = 0 at t = 0, then the exact solution is \tilde z = k×t, which does not satisfy equation 4.) In a recent paper (https://doi.org/10.1017/jfm.2021.1074, section 7) we referred to regions of a dry granular flow deposit that do satisfy (4) as "maximal", but this was not true of the entire deposit.
- **Figure 5 / line 209:** What is the source function Q_{in} for these solutions? Presumably the source is at a different value of x for each flux?
- **Line 235:** I don't fully understand *"an approximate analytical solution obtained by setting tan β = 0"*: why does tan β=0 allow an analytical solution, and what exactly is

being compared (is a numerical solution with $\beta \neq 0$ compared to an exact solution with $\beta = 0$?)

- **Figure 6:** Should *"(b,f,j) transverse deposit profiles"* read *"(d,h,l) transverse deposit profiles"*?
- **Table 1:** What is the order of convergence of the numerical scheme in space and time? From the time discretisation in equation (8), it appears to be first order in time. Is it also first order in space?
- **Figure 10 and 11:** these contour plots are noticeably slow to plot in my PDF viewer: are they particularly large figures that could be reduced in resolution?

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