Earth Surface Dynamics Discussions

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Comment on esurf-2022-31

Anonymous Referee #1

Referee comment on "The Entire Landslide Velocity" by Shiva P. Pudasaini, Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2022-31-RC1, 2022

In this manuscript, exact solutions to a recent model by Pudasaini & Krautblatter (2022) are presented. The model is a simplification of the widely used shallow-water approach for modeling shallow granular flows such as avalanches and landslides. The advance in this manuscript is to extend the range of exact solutions computed by Pudasaini & Krautblatter (2022) to decelerating flows, allowing a wider range of solutions to be calculated.

The model presented in this paper is considerably simpler than the shallow water models commonly used in both research and operational contexts for avalanche prediction and mitigation. The advantage of this simplicity is that explicit exact solutions can be found for avalanches that are steady or spatially-uniform, and implicit exact solutions can be found for flows varying in space and time.

While I am generally supportive of the approach of mathematical analysis of simple models, I have three major concerns about this work, which in my view mean that it is not suitable for publication in ESurf:

1. My primary concern is that the simplifications that have been made in the underlying model are so great, that the model is simply not relevant to real earth surface dynamics. I believe the model needs validation, including comparison of its predictions against the well-established shallow-water models and/or observations.

2. The paper appears to be aimed at practitioners who would benefit from a simple formula for avalanche velocity, but various aspects of the solutions presented appear make them unsuitable for this task.

3. Some of the results presented are not solutions of the governing equation, due to the exact solution method used being used beyond its point of validity.

Details of these points are given below:

1. The model is not a realistic description of natural avalanches and landslides

(a) The model used in this paper is independent of avalanche thickness. This means that the model predicts a landslide runout distance that is independent of the volume of material in the avalanche. This contradicts possibly the most fundamental observation of natural avalanches and landslides, namely that the runout distance (and avalanche velocity) increase with increasing avalanche volume. Consequently, I have serious concerns as to whether the work in this manuscript is of relevance to real avalanches or landslides.

(b) Gradients of the thickness of the avalanche enter into the model equations through the expression for alpha on line 95. However (though it is not stated explicitly) alpha is then assumed to be constant (or a prescribed piecewise-constant function). This is a very significant assumption that is not justified in either this manuscript or in Pudasaini & Krautblatter (2022). It differs significantly the dominant 'shallow-water' modeling approach where conservation of mass and momentum are used to determine how the thickness varies as a function of space and time.

Surprisingly, neither this manuscript nor Pudasaini & Krautblatter (2022) attempt to validate the new model. To validate the assumption of (piecewise) constant alpha, I would like to see comparison of numerical solutions of a shallow-water type model (e.g. equations 1 and 2 of Pudasaini & Krautblatter 2022) with the corresponding velocity equation (equation 5 of Pudasaini & Krautblatter 2022). The commonly-studied initial conditions of a 'dam-break' release of a finite mass of material could be used as one text case. Good agreement between the velocity fields and runout lengths of the two models would provide some reassurance that the assumption of constant alpha is reasonable.

(c) The origin of the term -beta u^2 in equation (1) is unclear. It is described as a viscous drag coefficient, but is not of the correct form for either a Newtonian viscosity, nor a Chezy or Voellmy drag (in the latter cases I would expect a term scaling with u^2/h, introducing the volume dependence mentioned in point 1(a)). What is the physical derivation of this term? Is it validated by any field or laboratory observations?

2. Setting aside the realism of the model, the exact solutions presented in this paper provide very limited value to the practitioners at which this paper appears to be aimed

(a) The model used in this paper is not predictive of avalanche thickness. This is an absolutely fundamental problem for using this model to assess landslide hazard (e.g. design protective structures, line 33), because the momentum and kinetic energy of a flow are proportional to the flow thickness.

(b) In the simple solutions presented in section 5.1, the parameters alpha and beta are given various constant values. How are these values chosen? (e.g. in line 263, what is the process to 'properly choose' the parameters? In particular, how is the free surface gradient chosen, given that it is spatially varying and can take any value?) The solutions of the model are clearly sensitive to the values of alpha and beta. If the model were to be applied to a real avalanche, how could the value of alpha and beta be found (as a function of distance downslope)?

(c) The model solutions in section 4 are not explicit: they rely on numerical solution of implicit algebraic equations (19) and (22). As such, the model equations presented here require numerical solution (and potentially, identification of multiple solutions), and therefore do not have the advantage of simplicity associated with explicit exact solutions.

(d) The introduction discusses the value of computing exact solutions to equations, and I am in agreement that exact solutions are have significant value for assessing models and numerical methods. However, the problems associated with solving full shallow-water models numerically (line 50) are overstated in this paper. Numerical methods for hyperbolic systems, such as that of Kurganov and Petrova https://doi.org/cms/1175797625, are robust and well validated, and have become very widely used. Importantly, they are not computationally expensive, and can find accurate numerical approximations to one-dimensional problems, such as those studied in this manuscript, within a few seconds. Numerical shallow-water calculations of this sort have been the primary tool used in operational avalanche hazard mapping for some years. The numerical shallow-water approach avoids many of the shortcomings of the present manuscript, in that it can predict both avalanche thickness and velocity, using a realistic rheology, and can be applied directly to real digital elevation model topography.

3. As noted in Pudasaini & Krautblatter 2022, the implicit equations 18, 19, 21, 22, have multiple solutions, and these are interpreted in this manuscript as a `folding' wave. This is incorrect mathematics: the solutions to equations (18,19,21,22) cease to become solutions to the governing PDE (equation 1) at the point that multiplicity of solutions starts (ie. when the gradient du/dx diverges). The `folding' process described in section 5.2.2 and 5.2.3 is therefore simply a mathematical artifact that occurs when the particular implicit solution method used in this paper is pushed beyond its point of validity. Therefore, the analogies made in section 5.2.3 between the shape of plots in figure 8, and folding depositional behavior in avalanche deposits are not valid. As the author is no doubt aware, solutions to shock-forming PDEs (such as equation (1) of this paper) only exist up to the formation of a shock, and require an additional equation (a jump condition) to be

integrated beyond this point.