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Discussions

Interactive comment on "Modeling rapid mass movements using the shallow water equations" *by* S. Hergarten and J. Robl

Anonymous Referee #1

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The authors propose a new set of model equations to describe granular mass movements down complex topography. The possible error associated with the Cartesian coordinates are said to be mathematically corrected for the large topographic gradients. Resulting balance equations are solved with the computational program, called GERRIS that is based on the operator splitting method. The results are tested with simple analytical solutions and numerical simulations with RAMMS. Just looking at the abstract, the readers would feel that it could have been made more scientific and professional. The writing is a bit awkward, English needs to be improved. The authors mention that: 'The overall results are in excellent agreement with the reference system RAMMS, and the deviations between the different models are far below the uncertainties in the determination of the relevant fluid parameters and involved avalanche vol-

umes in reality.' Then, one would wonder about the need of the proposed new model. It may be desirable to compare the performance of some new computational models with the existing one for some aspects. Are you trying to justify other tools/models rather than your own? This is what the readers would mostly feel while reading the manuscript. So, I think, when possible the performance of any new model should be tested against data. Any new/valid ideas should be welcomed, but that should be substantial, well formulated, consistent, compatible, clear and physically justified. Also it is not clear how to deal with the 'regional scale' simulations.

Some suggestions for possible considerations:

Title: 'shallow water equations': This does not necessarily indicate any particular contribution this MS might have brought to the audiences.

P6777:

L28: Would be better to point out applicabilities, and perhaps also some physical limitations, of the model used in RAMMS as mentioned, e.g., in Fischer et al. (2012).

P6778:

L1-5: It would be worth also mentioning other/alternative models, that could probably better describe mass flows, including Fischer at al. (2012), Pudasaini (2012), Pudasaini and Krautblatter (2014).

P6779:

L2: Pudasaini and Hutter (2003) has been extended, simulated and applied to data in Pudasaini et al. (2005, 2008) for the general curved and twisted channels.

L1-6: The previous restrictions/limitations in r.avalanche have now been overcome in the new developments with general model and the simulation tool: avaflow.org. This new project is particularly devoted to model developments and simulations for general mass flows in arbitrary mountain topography with erosion/deposition, geotechnical processes, and the simulation tool is based on a unified computational framework. So, this part of the text needs to be re-written.

L10-12: The way geometrical complications have been addressed in Fischer at al. (2012), with respect to the GIS data, is perhaps, one of the very desirable/practical ways to deal with the natural topography, and at the same time including the advanced mechanics (Pudasaini, 2012; Pudasaini and Krautblatter, 2014), and the geometry. The authors mention that: 'An extension taking the surface curvature into account for the price of more complicated differential equations was presented by Fischer et al. (2012)'. I don't think that this is appropriate. In fact, in Fischer et al. (2012), there is an innovative way to properly construct the curvature tensor, which, more physically, justifies the use of the viscous drag than in the classical Voellmy-Salm model. That is made quite clear in that paper by stating: 'Here, we fundamentally improve this model by mathematically and physically including the topographic curvature effects.'

L26: 'pressure gradient', and 'gradient of the water table': these terminologies are used in a confusing way.

P6780:

L1: Reference should be included for the equations.

L9: 'negative gradient of the water table': It can be negative or positive!

negative gradient of the water table –> gradient of the water table with respect to the horizontal reference datum.

L10: the density –> the bulk density

L12: shear stress: what is the shear direction? Along the surface of H or along x?

L15: 'the corresponding acceleration term': which one?

L17-23: - 'corresponds to the tangent of the slope angle ϕ': Not correct! Only true if $h_v = 0$ or h_v is parallel to H.

- Equation (3) seems to be a bit mixed-up! (2) is 2D, and (3) is somehow written as in 1D, and valid for h_v or its gradient being zero. About the last three-lines: with these multiplications, the previously defined pressure gradient (s) is now the actual downslope gravity force. And such derivations seem to fully ignore the variation of h_v (over H) along x. This ignores the actual hydraulic pressure gradient of the flowing fluid, i.e., $-gcos\phi(\partial h_v/\partial x)$ which would have been appeared in a properly derived model equations. It is clear that this extra term plays crucial and dominant dynamical role during the flow inception, flow obstacle interaction, and the deposition processes (Pudasaini et al., 2007; Domnik et al., 2013). So, such physically important aspect should be included in a dynamical model.

P6781:

L1-5: 'ψ is the inclination angle of the velocity': I don't think that this is well defined, and may not be physically meaningful. Otherwise, it needs to be justified. In a shallow flow the fluid moves along or parallel to the surface of slope, here ϕ.

L10: Not clear how you get '2' in $cos^2\phi$ in (5)? On the LHS of (5): did you also consistently multiply v_h by the relevant cosine factor? In total, there could be some inconsistency in multiplying and writing (5), please check!

L14-19: The authors did not mention any physical ground for these considerations. This is a critical aspect in developing new model equations. Also, friction is contained in τ. So, the readers may be confused.

P6782:

L1-4: Looks a bit strange operation! Or?

L13-15: 'Assuming that the bottom is parallel to the surface': Not clear what you want to say here.

How do you get $\cos^2\phi$?

P6783:

1. First, it is not clear the meaning of ψ, the consistency of the derivation and the correctness of (14). 2. For simple 1D downslope flow, as you mentioned before ($\psi = \phi$), then (14) could not be reduced to simple model (you did it later, but seems to be inconsistently!). 3. Compared to the formal transformations (e.g., Gray et al. 1999; Pudasaini and Hutter, 2003; Fischer et al., 2012) the terms in (...) in (14) should correspond to the centrifugal acceleration, Coulomb friction and velocity dependent drag. But, just for very simple situation, for the flow in a vertical plane, I couldn't see how (14) fits to any physically correctly derived model equations. E.g., from the simple physics, the first term in the bracket, the centrifugal force, must be associated with V^2. I think the authors should have justified how their model (14) captures the essential physics and how is this mathematically consistently derived. Their weakness seems to be seen in (22) which could not be deduced (as mentioned) for an inclined plane flow for which $\psi = \phi$. 4. Even if their math./physics is correct, it is not clear what is essentially new in this MS from the mathematical, physical and the mechanical point of view for the mass flow modelling compared to the existing advanced models? I would rather address these issues in detail and systematically with suitable figures to support the claimed new aspects in (14) so that the readers would realize/appreciate the new contributions.

P6784:

L1-4: In (14) there are 3 unknowns, but only 2 equations. Don't you need mass balance equation somewhere?

P6786: L3-19, and P6787: L1-5: These solutions have been derived in literature (e.g., Pudasaini and Hutter, 2007; equations (2.5)-(2.6); also by Voellmy, and reproduced in Pudasaini and Hutter, 2007; equations (2.8)-(2.15)). So, these are not new contributions, and referring the literature, only final solutions could have been utilized. I would have mentioned the source. The derivative operator is not properly used, should be

dv/dt. To obtain (20) you need to set $\psi = \phi$ (see P6781/L1-3). But, how do you formally get (20) from (14)? I couldn't obtain it. Based on your arguments, (14) implies (20) only if $\psi = 0$.

P6787:

L21-22: 'the initial flow depth of h = 1 m is still preserved in the main body'. This is not realistic! See comments on associated figure.

P6789:

L1: 'the effects of profile curvature': Curvature induces centrifugal force proportional to V^2 which is not seen in (14). Otherwise, discuss.

L6: 'with a smooth transition': where is the transition and how much?

L9: 'centrifugal acceleration of about 1 ms^{-2}': This force must be automatically generated by the topographic curvature (Pudasaini and Hutter, 2003, Pudasaini et al., 2005). Otherwise, please support your statement.

L13: 'the bulk mass of the avalanche remains undeformed': -This is unrealistic! - Why is there no lateral spreading? This is also not realistic. - The earth pressure coefficient may have substantial influence, e.g., in the deposition (Pudasaini and Kroener, 2008). The effect/discrepancy can easily be seen, e.g, in Fig. 4. It is worth discussing this aspect.

P6790:

It seems that the authors seem to say that their model is very similar to RAMMS but with artificially added centrifugal force. Then, the readers may wonder what is the need of the proposed model?

P6792:

It may be of basic interest to compare the simulation results of any newly constructed model with pre-existing model. But, what about validating with data and/or clearly showing some examples where new model solves some fundamental problems (may be at least in simulation for the time being) that other models could not do? Are you suggesting to use your new model, or any other pre-existing model?

L4: 'the differences found here are presumably not related to our approximation': How do you know that?

L18-28: The first models, simulations and validations for the flow of granular and debris material down generally curved and twisted channels have been presented in Pudasaini and Hutter (2003), Pudasaini et al. (2005, 2008). That seems to be relevant here.

'to the previous examples that are basically one-dimensional': You are confusing the readers! Before you solved geometrically 2D and 3D problems!

'our approach only applies corrections for large slopes to the longitudinal component of the velocity.': This is not realistic, e.g., for laterally confined flows (Pudasaini et al. (2005,2008). It should be discussed.

P6793:

L1-2: Again, such topographies are considered in Pudasaini et al. (2005, 2008) for simulations of debris avalanches and comparison with laboratory experiments.

L24-29: None of the models you mentioned actually include the real 2-directional curvature and twist of the channel as done in Pudasaini and Hutter (2003) and Pudasaini et al. (2005, 2008). The other models can only include lateral confinements by topographic pressures rather than intrinsic curvatures and twist.

P6794:

I think the momentum plot is redundant. There are some strange/confusing arguments that need to be properly presented. Again, do you want to suggest to use other similar

models or your model?

Conclusions: Based on the revision, this should be shortened and reformulated.

P6803:

There are several carelessness in the MS:

Initial uniform mass of 1 m normal to the slope in 350 m X 400 m, and Fig. 2a is said to be simulation result at t = 20 s (a relatively large time).

-What are inner and outer scales? What exactly is the position of the initial mass (in x, y coordinates)? - Solutions mentioned in Section 4.1 are valid for 1D flow and for all time constant flow depth parallel to the sliding surface, which is thus a mass point solution. But, simulation here in Fig. 2 is for deformable mass, as clearly seen in simulation (panel a). The simulation here is for 2D (or, geometrically 3D with lateral spreading). So, taking the vertical cut at $y = 0$ and comparing it with the 1D (or geometrically 2D) velocity solution of Section 4.1 does not seem to be compatible in the following respects: (i) in 2D there is lateral spreading, (ii) h is changing (fluid is deforming) during sliding/motion, (iii) exact solution is for non-deformable mass point motion in Lagrangian frame, while simulation is in Eulerian frame for deformable body. It is not clear how these results can be compared!

P6804:

Figure 3 looks great. But, as commented for Fig. 2, (i) comparison seems to be not compatible, (ii) these simple exact solutions are known for long time now. So, what is the significance of such figures and comparisons. This need to be justified/discussed.

Similar comments may also apply to other figures.

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