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

**Anonymous Referee #2**

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This manuscript describes the implementation of a Voellmy friction relation for the shallow water module of the open GERRIS flow solver. Simulation results of two different GERRIS approaches and the proprietary simulation toolbox RAMMS are compared to analytical solutions for a mass point model on simple topographies. Additionally one simulation example is shown for a complex topography. The presented ideas are interesting and fit well to the NHESS audience, especially the open source approach. However, motivation and description of employed correction terms often appear confusing and not straight forward to me; a proper motivation for friction and acceleration corrections (in particular based on a free surface gradient, i.e. $GERRIS_H$) would be desirable.



The authors utilize the given structure of a GERRIS flow solver module, which solves shallow water equations in a Cartesian framework and has been used for tsunami or similar simulations. Ad hoc correction terms, based on trigonometric relations, are used to implement an $acceleration\ reduction$ and a Voellmy friction relation. With this the model is utilized to simulation shallow gravitational flows on arbitrary topographies.

The manuscript is sufficiently well written and i find the supplementary material quite helpful. Following section two (which should be enhanced to verify what the authors are proposing) is quite hard without the additional material of the GERRIS flow solver (e.g. parameter file to check implementation since formulations in the paper are somewhat inconsistent with the implementation/parameter files). The simulation results are well described, but the correspondence and accuracy of agreement between the different models seems exaggerated. Although the simulation results look promising (and thus the approach may be technically justified), I am not convinced that the approach is justified in a mathematical/physical sense. However the manuscript is an interesting as proof of concept study.

Please find my detailed comments below.

**Line-by-line comments:**

- *6776, 12 '... commercial ...'* Maybe you should replace commercial by proprietary.
- *6776, 13 '... are in excellent agreement...'.* Excellent may be true for the comparison for the first comparison to $v_\infty$, elsewhere a bit exaggerated, see comments below.
- *6776, 14 '... the uncertainties in the determination of the relevant fluid parameters*

*and involved avalanche volumes in reality...'* This claim may be true but seems unjustified to me. As far as i see it, you did not quantify any uncertainties related to parameters or avalanche volume (which i agree is also not in the scope of this paper).

- *6778, 2 '... but peer-reviewed publications on technical details are still missing .'* What about e.g. Sampl and Zwinger (2004) or more recently Fischer et al. (2014) and references therein?
- *6778+79, 18–18.* Using same $x$ and $y$ for different types of coordinate systems in the same context is confusing here (see comments on Fig 1.), as reference see e.g. Bouchut and Westdickenberg (2004).
- *6779, 16. '....an overestimation of the acceleration...'* This is confusing. For me the basic problem seems: To account for the source terms including acceleration and friction for large topographic gradients OR in terms of implementation: the $tuning$ of the GERRIS toolbox to include those terms.
- *6780, Eq 1+5+10+14+15:* This is some kind of momentum balance but not shallow water equations as claimed in the text, compare e.g. different SW equation formulations in Popinet (2011) or elsewhere. Generally a component wise description would make it a lot easier to follow your equations and verify that what you have done is valid. I suggest you should consider rewriting this important section!
- *6780, Eq 2:* I suppose (especially to make the paper consistent) it should be $s = \nabla(A)$, where either $A = Z_b$ or $A = H = Z_b + h_v$ to distinguish between your two approaches GERRIS$_{Zb}$ and GERRIS$_H$ - if i get you right later on.
- *6780, Eq 3:* Here and elsewhere, why you do use the angle $\phi$ for the general slope correction and not $\phi_x$ and $\phi_y$ as component wise correction factors in your



equations (which are given in the $x$ and $y$ components respectively as the equations in the actual GERRIS implementation (considering the momentum balance in 2 space direction for the SW equations)). What would be the difference and why is your formulation valid?

- *6781, 1: '..for finite gradients..'* What is this? large gradients?
- *6781, 13: '.. but do not correct the terms of inertia due to surface curvature.'* I do not understand. I think you might be talking about the pressure gradients, where gravitational acceleration appears on the LHS of the SW equations (that can have also a curvature correction but in the sense of your paper should at least be corrected by $\cos^2\phi$ ?)?
- *6781, Eq. 4:* I see that this angle is the projection of the slope gradient on the actual velocity, but what does it actually mean? A sketch would be helpful.
- *6783, section 3, Equations 15–19:* To be consistent with the SW equations and what you actually implemented in GERRIS ($RapidMassMovement.gfs$) (which seems right to me in context of the implementation): $v_h$ **should be** $v_h\, h_v$ **in Eq. 17+18+19?**
- *6785, 20: '...n flow direction while leaving the lateral acceleration uncorrected...'* I am not sure that i understand this. What is $in\ flow\ direction$ and $lateral$ in Cartesian coordinates?
- *6785-94, section 4:'...run out, deposit...'* These terms have no direct meaning in terms of simulation results. You need to either define them (deposition = flow depth at time step ... run out = ...) or stick to the variables in terms of the physical model.
- *6790, 7:* Why do you use two different stopping criteria for the simulation instead of using a certain end time for GERRIS and RAMMS. Additionally a version info

on RAMMS would be helpful.

- *6790, 12 + Fig 5-7 + elsewhere '... unimportant for practical purposes':* I would say this statement is a bit generalized and it depends on the practical purpose. Looking at Fig 5-7 you can see deviations of about $10\%$ in flow height and $15-20\%$ for final flow velocities, which seems considerably high to me (also for practical application, i.e. dam planning)!!
- *6790, 22':* Where does the $cos$ factor arise from? Why not using the proper $v_\infty$?
- *6792, 7 '... for reducing oscillations...':* This might also depend on your DRY threshold that would be worth mentioning?
- *6792, 14 '... practically negligible...':* See comments above.
- *6793, 14 '... first-order features ...':* What are first order-features or the $order$, respectively?
- *6793, 25–30:* This paragraph is confusing me. Do the terms $transversal$ and $longitudinal$ make sense in a Cartesian framework? And how is transversal related to centripetal (which usually arise due to a curved track)? See also comment above.
- *6794, 4–13:* What exactly is the deposition shape, flow depth on last time step? You might compare apples to oranges considering the different stopping criteria, as you state yourself.
- *6794, 13–19:* This does not seem surprising, since larger gradients are expected for the free surface than the topography at the avalanche front. However, i (in general) do not understand the physical motivation to take the free surface gradient as reference for acceleration or friction. You should justify the $GERRIS_H$ approach.



- *6796, 8 'Even the resolution ...':* Also it would be helpful to mention the spatial resolution for the computation for the different simulation tools.
- *6796, 20 '... focused on snow avalanches ...':* I do not see this limitation in process specification. I see that the complex topography example you used is related to snow avalanches. However, everything else is related to models that are also used for debris flows or other gravitational mass flows.
- *6798, Supplement 'Multiply both components of the velocity by F according to Eq. (19)'* Are U and V not the momentum flux components, i.e. $h_v\, u_h$?

**Figures:**

- Fig. 1: This figure could be enhanced for a better understanding of the paper. Maybe you could include a sketch with your angles $\phi$ and $\psi$. Some axis labels would also be helpful talking about different coordinate systems.

### References


Bouchut, F. and Westdickenberg, M. (2004). Gravity driven shallow water models for arbitrary topography. *Communications in Mathematical Sciences*, 2(3):359–389.

Fischer, J. T., Fromm, R., Gauer, P., and Sovilla, B. (2014). Evaluation of probabilistic snow avalanche simulation ensembles with Doppler radar observations. *Cold Regions Science and Technology*, 97(0):151 – 158.

Popinet, S. (2011). Quadtree-adaptive tsunami modelling. *Ocean Dynamics*, 61(9):1261–1285.

Sampl, P. and Zwinger, T. (2004). Avalanche simulation with SAMOS. *Annals of Glaciology*, 38(1):393–398.


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