

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

S. Hergarten and J. Robl

joerg.robl@sbg.ac.at

Received and published: 11 February 2015

Dear Reviewer,

thank you very much for your thorough and constructive review of our manuscript. Your detailed comments and questions helped us to recognize at which points the readers might get stuck in the theory. We tried to write down the theory with a minimum mathematical formalism, but as is is not really simple, there are indeed some aspects deserving a more precise explanation.

From your comment “*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.*

C3374

GERRIS_H) would be desirable.”

we see that in particular the difference between our two implementations (and why this difference is not very large) needs some more explanation.

Responses to the line-by-line comments:

6776, 12 ‘... commercial ...’ *Maybe you should replace commercial by proprietary.*

Good idea!

6776, 13 ‘... are in excellent agreement ...’. *Excellent may be true for the comparison for the first comparison to v_{∞} , elsewhere a bit exaggerated, see comments below.*

Fair enough – we will replace the word “excellent” by a less euphoric word.

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).*

Well, you are right. The sentence was unfortunately worded. We will correct the abstract in a revised version.

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?*

Ok, we probably did not formulate this sentence well and we will clarify that. The mentioned sentence refers to the model ELBA. There are of course peer-reviewed publications describing SAMOS but as far as we know there are no peer-reviewed publications describing technical details of ELBA+. As you can see there are three citation related to SAMOS in our Paper.

6778+79, 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).

We do not think that it is really confusing as we do not transform them quantitatively in this paragraph. But we agree that a notation consistent, e.g. with that of Bouchut and Westdickenberg may help the reader. We have prepared a new version of Fig. 1 where the coordinate axes are labeled (as suggested below) with x, y, z, X, Y, Z , and X', Y', Z' , respectively, and will use it in the text consistently.

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.

Ok, we will explain this a bit in more detail here, although it is just the “opening” of the next section.

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!

There are indeed two different ways to write the shallow water equations, similarly to the original Navier-Stokes equations. The one we used here the is the “native” version directly referring to the acceleration. The advantage of this formulation is that it is readily obtained from the Navier-Stokes equations with the approximations mentioned in the lines before. But you are right, most of the literature, in particular when focusing on numerics, uses the conservative (also called advective) version. For our purpose, the advantage of the “native” version is that our arguments on the acceleration can immediately be incorporated, so that we thought we would get along without the conservative version at all. But you are right that GERRIS uses the conservative version,

C3376

and we in principle need it to describe the implementation formally correct. So we will also introduce the conservative version here for those readers being more familiar with it. Concerning a component-wise description we are not completely convinced. It is perhaps slightly simpler for those readers not so familiar with vector analysis in the conservative version. But since all our modifications concern the lengths of the respective vectors would be a bit cumbersome in components. We will think about this point again.

6780, Eq 2: I suppose (especially to make the paper consistent) it should be $s = \nabla(A)$, where either $A = Zb$ or $A = H = Zb + h_v$ to distinguish between your two approaches GERRIS_{Zb} and GERRIS_H – if i get you right later on.

Not completely, the difference between the two GERRIS implementations is more complicated. In the original shallow water equations and, e.g., in the Savage-Hutter and Bouchut–Westdickenberg models, the acceleration always depends on the gradient of the fluid surface (at least up to first order). Otherwise the models would be poor for lake-like structures. The same holds for both implementations presented here. The difference between GERRIS_{Zb} and GERRIS_H only concerns the correction terms for the situations where the gradient of the fluid surface becomes large. This is explained in the last section on page 6784, but we will try to explain it more clearly in a revised version. Beyond this, we agree that it would be better to replace H with z_b and then use $H = z_b + h_v$ for the fluid surface in order to be consistent with the GERRIS implementation.

6780, Eq 3: Here and elsewhere, why you do use the angle for the general slope correction and not φ_x and φ_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?

All corrections (to acceleration or velocity) that we introduce must maintain the direction

C3377

of the respective vectors, so that both components must be multiplied by the same factor. Here where we consider the “downslope” acceleration, this acceleration must still act in direction of the steepest descent, which would not be the case when using different angles. This is one of the reasons why we are not convinced that writing the equations in components instead of the vector formalism would be helpful.

6781, 1: ‘... for finite gradients ...’ What is this? large gradients?

It should have been the opposite of “infinitesimal”, i.e., valid beyond the first order. We will try to clarify it.

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 $\cos^2 \varphi$)?

Admittedly, this point is indeed difficult to understand here. It is considered in the introduction to Sect. 4 and in detail in Sect. 4.2. Imagine flow over a curved topography with gravity and friction being switched off (but the fluid still tied to the surface, not lifting up). In reality, the absolute value of the (3D) velocity (= velocity parallel to the surface) remains constant then, but in our approach, the horizontal component would be constant, leading to changes in the component parallel to the surface. In Sect. 4.2 it is shown that this effect is rapidly compensated by the acceleration/deceleration and thus not serious. Maybe it is better to remove this sentence here as it is discussed more thoroughly in the introduction of Sect. 4 and in Sect. 4.2.

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.

Basically it is just projecting the frictional acceleration that originally acts in direction opposite to the (3D) velocity to the horizontal plane. While $\tan \varphi$ is the overall slope

C3378

of the fluid surface, $\tan \psi$ is the slope in direction of the velocity. We will improve the explanation and have included the angles φ and ψ in the new Fig. 1(b).

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?

You are right, thanks! In order to avoid the conservative version of the shallow water equations we wrote the operator splitting scheme here for the “native” version. Eq. (15) must be replaced by the conservative version written in terms of the momentum $q = h_v v_h$ (with $f(\dots)q$ as the last term). Eq. (16) remains, and Eqs. (17)-(19) will be the same with q instead instead of v_h .

6785, 20: ‘... in 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?

It is just the approximation introduced in Eqs. (8) and (9). We derived a correction term for the acceleration and then consider only its projection on the velocity. This means that our correction introduced for large gradients only acts in direction parallel to the velocity, while an overestimation of the acceleration in lateral direction remains. We will explain both approximations here in more detail.

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.

You are basically right: Terms like “run out distance” or “deposition” are frequently used in the natural hazards community, but cannot directly be obtained from the model results without a clear definition. We will give a definition in terms of model variables in a revised version of the manuscript.

6790, 7: Why do you use two different stopping criteria for the simulation instead of us-

C3379

ing a certain end time for GERRIS and RAMMS. Additionally a version info on RAMMS would be helpful.

Thank you for this comment! This was not an ideal choice, although in both models (RAMMS and GERRIS) there is not significant fluid motion at the end of the simulations. However, as the license of RAMMS has expired and there is no budget to purchase a license for another year. We have recalculated the GERRIS simulations for the RAMMS default stopping criteria. This clearly shows the value of a freely available code for the natural hazards community to describe rapid mass movements on general topography! As you can see on the new Figure 8, results did not change significantly and there are only very small deviation in the depositional zone at the valley floor. The stopping criteria of RAMMS and GERRIS are now consistent. We will change also the description in the text.

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)!!

We still are convinced that the comparison of model results from RAMMS and GERRIS shows a very good agreement, except the deviations described explicitly and justified in the text. Even if we superimpose the results (time slices) of GERRIS and RAMMS on top of each other (see uploaded Figure) there is only a slight shift in the “timing” between the two different models which may have caused a deviation in flow height and velocity of 10% at a specific point in time. By forcing GERRIS towards smaller time increments this temporal offset can be reduced. However, in natural hazard mitigation projects it is not crucial whether an avalanches reaches one second earlier or later. As we all know, relevant parameters are maximum flow height, maximum velocity, maximum pressure or maximum momentum and they are, as shown by the hull of the results in very good agreement between the two models. We will clarify this point in a

C3380

revised version of the paper.

Deviations already described in the Paper: Deviations in flow height and velocity are observed in the concave test case, as the maximum height of the deposit is slightly shifted between RAMMS and GERRIS. However, velocity and therefore momentum of the mass movement are small in this section of the flow path. Deviations between the two models are also observed in the convex test case just below the terrain edge and at the moment the avalanche front decays due to ongoing stretching. The latter deviation is caused by strong oscillations in flow height and velocity of the model RAMMS and points out a numerical issue of RAMMS. Deviations behind the terrain edge are caused by the recalculation of the horizontal components of the velocity vector to slope parallel velocities – a limitation of our model that is explained and even justified in detail in the paper.

Rapid mass movements in the depositional domain of the flow path are characterized by very low flow velocities. A planned dam is always higher than the maximum flow depth but in general much higher because of the momentum of the avalanche (debris flow, ...). Therefore it might not significantly affect the planning whether the position of the maximum depositional height is shifted 20 m valley side or not. In case of terrain edges in the convex test case: During many years of natural hazard projects we have never seen dams located immediately below convex terrain edges. However, planning dams or other protection measures require a lot of expert-based decisions, knowledge and experience, and numerical models like the one presented in this study represent just one important tool!

6790, 22 ‘Where does the cos factor arise from? Why not using the proper v_{∞} ?’

There is nothing explicitly used here, it is just the effect of considering only the horizontal component of the velocity. The avalanche comes in at 18 m/s at 30° slope, which means that the horizontal velocity is $18 \text{ m/s} \times \cos 30^{\circ} = 15.6 \text{ m/s}$. This velocity persists, so that the “real” (3D) velocity is $15.6 \text{ m/s} / \cos 45^{\circ} = 21.7 \text{ m/s}$ immediately

C3381

after the transition. We will explain it in more detail because it seems to be helpful for understanding the approximation with the horizontal velocity.

6792, 7 '*... for reducing oscillations ...*': *This might also depend on your DRY threshold that would be worth mentioning?*

The DRY threshold is used in both GERRIS approaches: GERRIS_H and GERRIS_{ZB} and the DRY threshold was always $DRY = 1E - 3$. Nevertheless we observe strong oscillations similar to RAMMS (also using a similar threshold) in GERRIS_H and only minor oscillations in GERRIS_{ZB}. Thus, we are quite sure that the DRY threshold do not cause or prevent the observed oscillations. However, we will add a comment line in the supplementary GERRIS parameter file, describing the DRY threshold.

6792, 14 '*... practically negligible...*': *See comments above.*

Please see justification above.

6793, 8 '*... first-order features ...*': *What are first order-features or the order, respectively?*

You are right – this is a fuzzy description. We will correct this.

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.*

We will try to explain this better in the introduction of Sect. 4, so that it also should become clearer here.

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.*

Ok, we will defines these terms more precisely in a revised version. We have changed the stopping criteria of GERRIS to be consistent with RAMMS now and have recal-

C3382

culated Figure 8. Comparing different models is always like comparing apples with oranges – However at least both are delicious fruits ;-).

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.*

No, it is not so simple. As discussed above, both GERRIS-based versions as well as all other models derive the driving force from the slope of the fluid surface. Therefore, GERRIS_H where the corrections are also derived from the fluid surface is in principle the “native” and more consistent version.

6796, 8 '*Even the resolution ...*': *Also it would be helpful to mention the spatial resolution for the computation for the different simulation tools.*

Ok, we will add information on mesh type and resolution.

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.*

You are absolutely right. Our approach can be used to describe several types of rapid mass movements on general topography. Only the last example describes a dense snow avalanche with characteristic rheological parameters for this type of rapid mass movement. We have just compared the results of our approach with the RAMMS::AVALANCHE module. We will rewrite the particular sentence in a revised version.

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. hv uh?*

Yes, see comment above. This will also be fixed.

C3383

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

Good suggestion, we have prepared a new version of Fig. 1 taking this aspect and the labeling of the axes suggested above into account.

Best regards

Stefan Hergarten and Jörg Robl

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 6775, 2014.

C3384

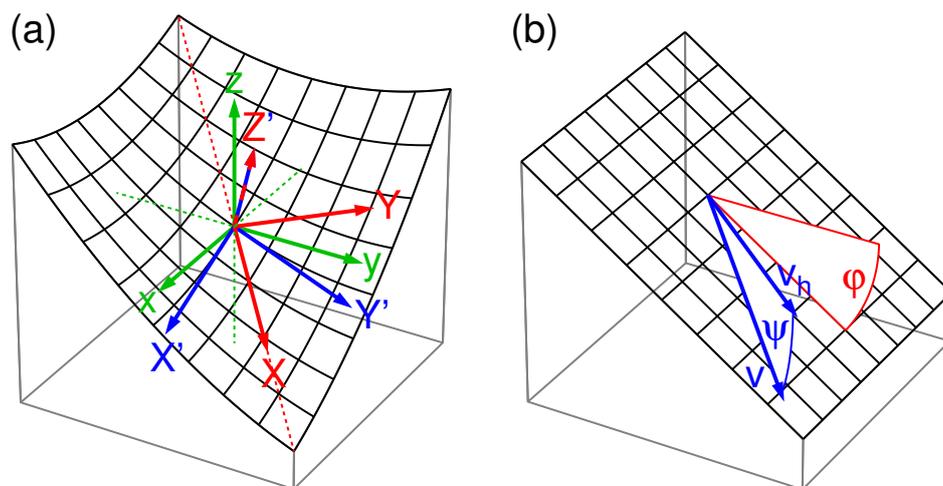


Fig. 1. Revised version of Figure 1

C3385

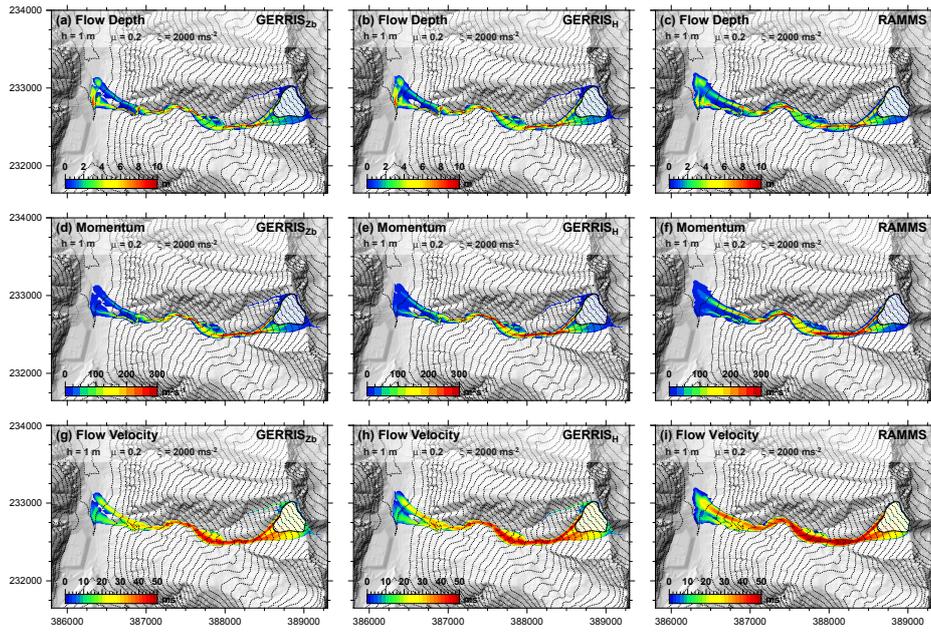


Fig. 2. Revised version of Figure 8

C3386

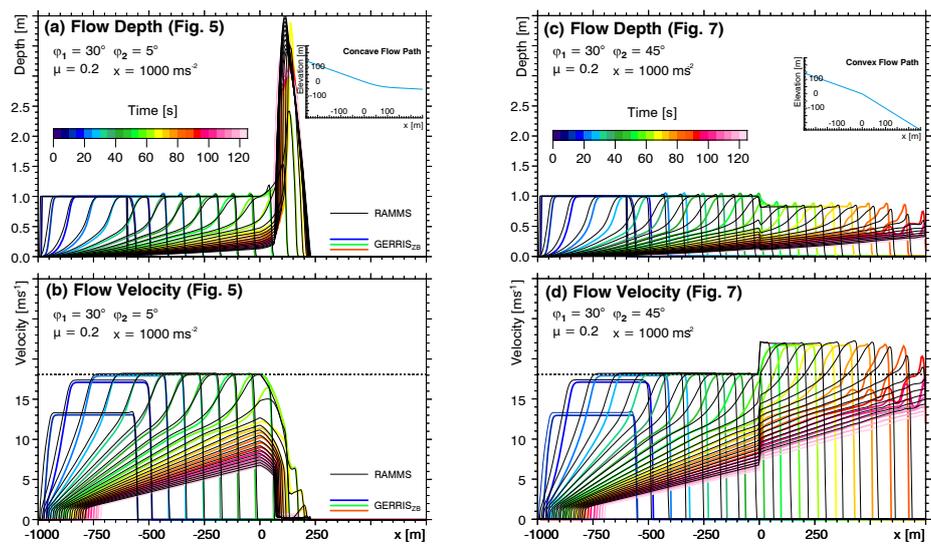


Fig. 3. Direct comparison RAMMS / GERRIS_ZB of figure 5 and figure 7

C3387