Nat. Hazards Earth Syst. Sci. Discuss., 2, C3312–C3329, 2015
www.nat-hazards-earth-syst-sci-discuss.net/2/C3312/2015/

Natural Hazards and Earth System Sciences
Discussions

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: 6 February 2015

Dear Reviewer,

thank you for your thorough review of our manuscript. While finding some of your comments helpful, we feel that large parts your review go into a direction which is clearly not within the focus of our paper. There is a quite small community working on improvements of the physics behind granular mass movements e.g., by two phase flow, lubrication, internal mass and momentum exchanges. We do, of course, not mind to give more credit to this work, but our target audience is a completely different one. We do not introduce more elaborate physics compared to previous models, in particular compared to the widely used proprietary model RAMMS (see the previous discussion

in this forum with one of the developers), but present a quite simple solution for practical application.

So please let us be honest about what is ready-to-use! In 2012, M. Mergili et al. (NHESS 12, 187–200, 2012) concluded: "Since no user-friendly Open Source software for the motion of granular flows (fully incorporating the relevant physical and geometrical processes) is available at present, the further development . . . ". What has changed so far, except for this first attempt by M. Mergili et al.? Later on in your review you mention the research project avaflow.org. After being surprised about your comment, we revisited the project's web-page without finding anything in this direction beyond r.avaflow being presented in the paper mentioned above. There are no publications about novel numerical models and there are no already available numerical codes for use beyond the model published Mergili et al. (NHESS 12, 187–200, 2012). You mention, e.g., the recent paper by Pudasaini and Krautblatter (2014) which may indeed be forefront science on the physical description of granular flow processes. But how far off is the one-dimensional numerical implementation of this sophisticated model from being applicable to a general topography, and when will parameters be calibrated for being used in mitigation projects against natural hazards?

From your point of view – presumably focusing on the representation of the physical processes – you question the need for one more model. However, we are convinced that the majority of the potential users will not share this opinion. One of the reasons why we are quite sure about this is that one of us has been involved in a series of projects developing mitigation strategies against natural hazards during the last decade, partly outside university.

Although we think that we have clearly explained in the manuscript why our approach is of broad interest for the natural hazards community, let us state the most important arguments again:

- The physics behind our approach is not new, it is just Voellmy's rheology that has been used in the proprietary system RAMMS where much effort has been spent for calibrating the parameters for snow avalanches. Therefore, parameters defining the flow resistance law are fully compatible between RAMMS and our approach and results can be directly compared. This may not be important in basic research but is crucial whenever numerical codes are used to tackle real world problems where authorities are involved.

- We present a NOVEL numerical approach that successfully describes rapid mass movements by standard shallow water equations in Cartesian coordinates with additional correction terms for large topographic gradients, but NOT a new code. Almost any code for solving these shallow water equations can be used for an implementation of our approach, provided that the law of friction can be defined by the user. A variety of such codes, partly open-source, has spread since the 2004 Indian Ocean tsunami. The advantage of this concept is obvious:

  1. The user can select an available code according to the own preferences, e.g., being freely available, providing highly efficient state-of-the-art numerics including (e.g., shock capturing schemes and adaptive mesh refinement), providing support by a large community or being available for a given operating system.
  2. The user can decide between the Voellmy rheology that has been successfully applied and calibrated from snow avalanches for decades and more sophisticated flow laws. Depending on the numerical code used, this may not require low-level programming.
  3. The approach in itself will not become outdated so easily, while any non-commercial specific software will probably be as soon as PhD students or postdocs have gone.

Under these aspects it should be clear why we validate our approach against the most widely used, but proprietary model RAMMS as a reference. There are no well-defined

benchmarks in this field. And we simply cannot follow your reasoning when saying that "the readers will wonder whether we are trying to justify other tools/models rather than your own". We neither validate nor refute any other model, but only point out a novel way to simulate the physics included in the established reference model using general fluid dynamics software packages. Then we use a particular software (GERRIS) and a sequence of examples with increasing complexity for illustrating that it indeed works. As the second reviewer states, this is a proof of concept.

You also comment on the style of writing. Taking into account the readership of NHESS and comparing it to the papers you mention in your review, we do not believe that it is fruitful to discuss this topic in depth here.

In your detailed comments you raise some questions on the correctness of the derivations. We intentionally decided for a way to write down the theory with a quite small number of equations. The price of this is that not all equations are self-explanatory without reading the text in between. Without attempting to offend you, we have the impression that you tried this at least at a few points. After checking all steps again we can assure you that the derivation is correct, and that the steps are not as unclear as you state.

From some comments we even got the impression that there were some fundamental problems in understanding the idea behind this approach. The reader could even think that you see it just as a wrong implementation of the theory proposed by Fischer et al. (2012). To make it clear again: We do not consider effects of curvature at all, in particular not centrifugal forces. It is just extending the shallow water equations in Cartesian coordinates towards steeper topography, i.e., overcoming the basic limitation of these equations, and then bringing it back to a form where standard algorithms for solving the equations numerically can be used. We believe that the misunderstanding is a specific problem arising from your deep knowledge on the way this problem is usually tackled in curvilinear coordinate systems, so that we even believe that you started reading the manuscript a bit in a wrong direction from the beginning. We are quite confident that the reader with a lower or average background on this will not run into this problem.

Concerning your rather comprehensive annotations:

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

This is true, so we should perhaps add "in Cartesian coordinates" and eventually "by introducing appropriate friction terms".

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).

We can, of course, mention that some limitations of RAMMS have been discussed in this paper. But beyond this, we think that we described the contribution of this paper correctly. In particular, we prefer not to contribute to the discussion how significant these limitations and the extensions suggested by Fischer et al. are in practical applications.

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).

Yes, we do not mind mentioning these models here.

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.

Of course we do not have any problems with mentioning this here.

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.

As you mention, avaflow.org is a research project. But as mentioned above we revisited the project's web-page without finding anything in this direction beyond r.avaflow published M. Mergili et al. (NHESS 12, 187–200, 2012). We hope that you agree that a personal communication of an anonymous reviewer may be a rather weak reference and not satisfying the readers.

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.'

After reading this comment we even thought that we might have misunderstood the paper of Fischer et al. in its spirit. But after considering it again we are still convinced that it just takes into account the curvature of the surface and the resulting centrifugal force. I hope you agree that it has been clear in Voellmy's law from the beginning that the "dry friction" is proportional to the force normal to the bottom, and that this, of course, theoretically requires taking into account the centrifugal force if it is strong. We therefore strongly disagree that the paper of Fischer et al. improves the physics behind Voellmy's model. And, as stated above, we do not want to comment on the practical relevance of the additional term and whether it is fundamental as said in that paper.

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

To our understanding, the idea behind the shallow water equations is exactly as written, and the terms "horizontal pressure gradient" and "gradient of the water table" should be clear.

P6780:

L1: Reference should be included for the equations.

Following the mathematical literature on the shallow water equations we think that these equations are old enough (more than 150 years) to be used without a detailed reference. However, we can insert a reference to the book of Vreugdenhil (1994), although the average reader will find more compact and useful information on these equations at Wikipedia.

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.

Being a vector, the gradient can be neither positive nor negative. To our knowledge, the term "negative gradient" for the vector opposite to the gradient, i.e., in direction of steepest descent, is not unusual (e.g., http://en.wikipedia.org/wiki/Gradient_descent). Those readers who do not understand it will have to go back 5 lines to Eq. (2) where it is formally written.

L10: the density → the bulk density

Ok, We wil change that term.

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

Be assured that we thought quite much about describing the shallow water equations in a simple, but basically correct form. We wrote that the friction term is opposite to the velocity (which is horizontal), and that the friction term is written IN TERMS OF A basal shear stress. It is, of course, horizontal and not parallel to the flow bed, but we did not claim that τ is the real shear stress at any surface.

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

"If the gradient of the water table is large, the corresponding acceleration term . . . ", and the previous paragraph explains how the acceleration depends on the gradient of the water table. So is it really so unclear?

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

and

- Equation (3) seems to be a bit mixed-up! (2) is 2D, and (3) is somehow written as in 1D, and valid for hv 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 hv (over H) along x. This ignores the actual hydraulic pressure gradient of the flowing fluid, i.e., $-g\cos\varphi\frac{\partial h_v}{\partial x}$) 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.

When describing the shallow water equations we explained that the acceleration results from the gradient of the water table. Therefore it should be clear that our arguments on the slope angle refer to the slope angle of this water table and not to the bed.

We will state this explicitly here again, and we will also mention that the term $\cos\varphi$ arising from (ii) is basically the same as in the Savage-Hutter model and in the Bouchut-Westdickenberg model. So the terms you mentioned are definitely not disregarded as you presume!

And the mixing of 1D and 2D is also part of making the theory as short and as simple as possible. We need the 2D shallow water equations, but the arguments on the acceleration given in this section only affect its absolute value and not its direction. When thinking of a rigid body on an inclined plane the average reader should be able to follow the argument with $\sin\varphi$ instead of $\tan\varphi$ (ii) and the additional factor $\cos\varphi$ arising from the extraction of the horizontal component of the acceleration. Writing all this in 2D using the norm of the gradient vector would clearly not helpful for the majority of the readers.

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.

It is well defined in Eq. (4) in this paragraph. Mathematically, it is the projection of the gradient of the water table on the direction of the (horizontal) velocity. And your argument on the physical mean is wrong: Flow velocity is, of course, roughly in the plane of the water table, but there is still a degree of freedom. It can be in direction of the steepest descent (then $\psi = \varphi$), but also be along the contour lines (then $\psi = 0$), and also be in any other direction within this plane.

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

It is stated that Eq. (5) stems from the three corrections introduced above, and it was

written that each of the corrections (i) and (ii) introduced a factor $\cos\varphi$ to the acceleration. However, we will add a note on this in order to point out this more clearly.

Concerning the second point: Our derivation started from the original shallow water equations where only the horizontal component of the velocities has been considered from the beginning. One could, of course, also start from an inclined coordinate system (as in the Savage-Hutter model), split gravity, and multiply everything in the entire equation by $\cos\varphi$ in order to project the velocities to the horizontal plane. Then, the velocities are multiplied by $\cos\varphi$, but the gradient operator transforms inversely (with $\frac{1}{\cos\varphi}$), so that it is consistent. One would, of course, exactly arrive at the same result.

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.

There cannot be a physical reason as it is just a mathematical matter. We have shown that applying the shallow water equations without any further consideration leads to an overestimate of the acceleration. Then we decided to leave the acceleration as it is (in order to maintain the basic structure of the shallow water equations) and to compensate it by an artificial acceleration given in Eq. (7). This artificial acceleration is considered as an additional friction term, but it remains a geometric term without changing anything on the physics.

P6782:

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

Mathematically or physically? Mathematically it is nothing but a projection: For any vector $\vec{a}$ and any unit vector $\vec{e}$, $(\vec{a}\cdot\vec{e})\vec{e}$ is the projection of $\vec{a}$ on $\vec{e}$. Physically it is just an approximation: The acceleration is overestimated at large slope gradients, and we only apply a correction to its longitudinal component as this component immediately affects the absolute value of the velocity. The effect of neglecting the correction in transversal direction is discussed in the validation section.

L13-15: 'Assuming that the bottom is parallel to the surface': Not clear what you want to say here. How do you get $\cos^2\varphi$?

Ok, we agree that some more explanation is helpful. One of the factors $\cos\varphi$ arises from the use of h_v instead of h normal to the surface, and the other one from the projection of gravity. The second one is, of course, the same as in the Savage-Hutter model and in RAMMS. The reader may easily recognize it when imagining a rigid body on an inclined plane. And from this explanation it is clear that one of the two $\cos\varphi$ factors, should refer to the bed topography instead of the fluid surface. We will add a short explanation on this.

P6783:

1. First, it is not clear the meaning of ψ, the consistency of the derivation and the correctness of (14).

The meaning of ψ has been explained, and the step to Eq. (14) is nothing but basic algebraic calculations.

2. For simple 1D downslope flow, as you mentioned before ($\psi = \varphi$), then (14) could not be reduced to simple model (you did it later, but seems to be inconsistently!).

Eq. (14) in 1D with $\psi = \varphi$ turns into

$$\frac{\partial}{\partial t}v_h + (v_h\nabla)\,v_h = g\left(\sin\varphi\cos\varphi - \mu\cos^2\varphi - \frac{v_h^2}{\xi h_v\cos^2\varphi}\right).$$

When switching to Lagrangian coordinates and replacing v_h and h_v correctly by v and h, respectively, we EXACTLY arrive at Eq. (22).

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 $\psi = \varphi$.

This is wrong: It was clearly stated that effects of curvature are not considered. So we do not consider centrifugal forces. This could, of course, be included additionally, then there would be one more term. The first term is the correction to the shallow water equations at large gradients and not according to curvature.

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.

This is discussed in some detail in the general part of our response.

P6784:

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

Did you ever see any attempt to solve the shallow water equations or any other form of the Navier-Stokes equations without a mass balance? It is obvious that the mass balance in the original form considered as part of the shallow water equations (in terms of h_v and v_h) remains valid. However, to make it clear, we will add a sentence on this.

P6786:

L3-19, and P6787: L1-5: These solutions have been derived in literature (e.g., Pu-

dasaini 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.

Yes, indeed. Admittedly we found these solutions so simple that we did not look for sources. And we did not even think about the question whether this almost trivial part would be considered as a new contribution. We will, of course, give reference to a source.

The derivative operator is not properly used, should be dv/dt.

Ok, may be better, although it was stated that it is in Lagrangian coordinates where $\frac{\partial}{\partial t} = \frac{d}{dt}$.

To obtain (20) you need to set $\psi = \varphi$ (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 $\psi = 0$.

First step: $s = \tan\psi$ in 1D. Then we combine gs and the first term in the brackets to $s\cos\varphi\sin\varphi$. Next step is dividing everything by $\cos\varphi$ in order to switch from v_h to v at the left-hand side. Then the only unresolved term should be $\cos^3\varphi$ in the denominator of the last term, and this one should be consumed when converting v_h to v and h_v and h. So please let us know if you obtain a different result here as this has been one of the basic tests for ourselves to be sure that our equations are correct.

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.

If the initial mass is long and wide enough (which is obviously the case here), the initial flow depth will be practically preserved in the region around the center, similarly to diffusion plus advection.

P6789:

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

We clearly stated that this centrifugal force is not considered at all, and we used a version of RAMMS also disregarding centrifugal forces. As discussed above, we are not completely convinced that this centrifugal forces are practically as important as pointed out by Fischer et al. (2012).

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

and

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.

Again, we do not add an additional centrifugal force! We just define the curvature of the synthetic topography so that an avalanche entering from the upper ramp with the terminal velocity is exposed to a centrifugal acceleration of about 1 ms^{-2}. The transition zone is represented by a parabolic profile where the curvature at zero slope (i.e., first order) exactly produces the given centrifugal acceleration at the theoretical terminal velocity.

We guess that it is the same as in the literature you mention, but it is so trivial and unimportant here that it makes no sense to write anything on this.

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.

Lateral spreading has been discussed above and the profile where taken along the central line. So it is indeed unimportant over a quite long time.

You may have noticed that our formulation does not include any corrections for non-shallow flow by earth pressure coefficients, but it should be obvious to the reader that they can be included like any other modification of the rheology. When comparing to RAMMS, this was of course treated the same way, and any discussion on the importance of these coefficients would just diffuse the focus of this paper.

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?

We hope that it only seems (taken to the power of two) so because it is shallow water in Cartesian coordinates and not with centrifugal force. If it was, it would be just a bad copy of Fischer et al. (2012). We seriously hope that you did not see it that way.

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?

As stated in the conclusions, we would suggest potential users with some basic background in computing to use our model if they, e.g., want to test other rheologies, model several hundred avalanches or debris flows in batch mode, perform Monte Carlos simulations to investigate entire domains in parameter space, if they just do not have the money for purchasing a license of RAMMS, or if they do not want to wait, e.g., for the results of avaflow.org.

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

We thought that the word "presumably" expresses that we have arguments in favor of this hypothesis, but cannot prove it. The arguments on the discretization schemes given below would explain the observed differences in a consistent way, but we did not track this rather small effect in detail so far.

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.

Yes, ok, we will cite the studies in this context.

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

Ok, we could replace "basically" by "in their spirit". However, it should be clear that our examples could in principle be treated as 1D problems (as well as the analytical solutions), but are numerically considered in 2D with a sufficiently wide avalanche body.

'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.

"Velocity" should, of course, be "acceleration", sorry for this mistake. But beyond this, laterally confined flow is in fact the situation where the gradient of the fluid surface in direction normal to the flow velocity is small, i.e., where our approximation works perfectly. Deviations only occur in case of strong curvature, and these deviations are discussed in the last example.

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.

Maybe, but what is the use of this arguments apart from being cited once more?

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?

It is not redundant because the diagram displays maximum values, and $\max(hv) \neq \max(h)\,\max(v)$ in general. The rest of this comment should have been answered sufficiently above.

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

Based on the discussions above, we have the impression that the reviewer misunderstood (a) crucial aspects of the numerical formulation and (b) the scope of the study: to offer a free and flexible tool to describe rapid mass movements on general topography. We will clarify some points as described above.

P6803:

There are several carelessness in the MS:

and

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

and

-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!

We think that these aspects have been discussed above sufficiently.

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

We think that these aspects have also been discussed above sufficiently.

best regards

Stefan Hergarten

Jörg Robl

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