r.avaflow v4, a multi-purpose landslide simulation framework

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Response to the Editor

Based on the positive responses to your article, I encourage you to make the appropriate revisions and submit a revised manuscript.

We would like to thank the editor for handling our manuscript and for giving us the opportunity to submit a revised version. Below, we provide the detailed responses to the comments of the two reviewers.

We note that we have also made some additional changes mainly to Section 2.2., in order to enhance clarity and consistency.

Response to Reviewer 1

I have read and review the manuscript r.avaflow v4, a multi-purpose landslide simulation framework with great enthusiasm.

It represents a great innovation in the landslide modelling field, together with an open source code where everybody is able to apply these innovations for analysis and assessment. Overall a great read, and a major landmark paper in the landslide modelling field.

It should definitely be published by the journal, and I will personally use the contents in further research substantially.

It features a thorough testing of the software and model implementation, with detailed considerations, thorough testing and case studies, as well as illustrative figures. Though, some of the derivations seem to be derived somewhat idealistic.

We would like to thank the reviewer for the careful reading of the manuscript and for the appreciation of our work. Please find below our responses in blue (where we agree with the comments) or orange (where we do not agree). Major changes in regard to the final response to the open discussion are written in purple colour.

I do have some suggestions for the authors to adapt and perhaps improve the text, which are given below:

Introduction: It might be good to get a single paragraph describing also the state of other modelling developments, and how these deal with these issues. For most, r.avaflow4 seems a unique and novel solution, but perhaps not all. There are windows-based systems with interfaces, with nicer visualization. The challenges are now perhaps a bit too focused on r.avaflow4

We agree with the reviewer, there are of course other very relevant initiatives and software packages. We have included a paragraph at the start of the Introduction in order to set the scene:

"Mass flow simulation tools are used for numerically reconstructing, predicting, and communicating flow-type landslide processes. The first conceptual model for mass flows dates from the 1930s (Heim, 1932), and the first physically-based approach from the 1950s (Voellmy, 1955). More sophisticated models have evolved from the earlier models over time. By the mid-2020s, many practitioners still prefer physically rather simple models put into user-friendly interfaces, such as the RAMMS software (Christen et al., 2010). At the same time, scientific attention is paid to two-and multi-phase models such as OpenLISEM, integrating slope hydraulics, landslide release, landslide, and landslide runout (Bout et al., 2021) or r.avaflow, a flexible multi-phase computational framework focused on complex, cascading landslide processes, and on a full 3D implementation of the material point method (Cicoira et al., 2022). We focus on r.avaflow, …"

In addition, we have included the following sentence in the last paragraph of the Introduction chapter:

"We note that for (2), (3), and (5), we are not aware of any comparable software tools appropriately dealing with the challenges mentioned."

Line 55: with rather unsuccessful earlier attempts to create a powerful Windows-based stand-alone version of the tool – I would remove this, you are offering a great tool, and the fact this didn't work out shouldn't matter to the reader here.

We agree with this comment and have removed this sentence.

Line 67: I would rewrite this a bit (Basically, it transports mass and momentum of up to three phases through a regular grid. -> The model applies physically-based equations to estimate and calculate numerically the motion of mass and momentum of up to three phases...)

Thanks for this suggestion. We have largely picked up the suggestion and reformulated the sentence as follows:

"The model applies physically-based equations to numerically estimate and calculate the transport of mass and momentum of up to three phases through a regular grid."

2.2: I get the continuity derived basic advection framework of the equations is a bit duplicate, but it might be good to explicitly define the three phases at least for the reader here., with a small figure with definitions of phases, velocities/grid

After some discussion, we rather think that an additional figure would be beyond the scope of this paper. The arrangement of the phases is shown in Fig. 1. The advection framework is relatively complex, and a figure would require a lot of additional explanations making the article quite lengthy. Therefore, we rather prefer to direct the readers to Wang et al. (2004), where the advection framework is described in all detail (for one phase, but works in a similar way for two or three phases).

Line 105: Will there be a warning? Or is the scope of realistic parameter combinations difficult to detect (That seems very possible to me, although basic errors should be simple)

This is a very good point. In fact, there are no warnings, only specific error messages if the input is formally incorrect. Warnings in case of inappropriate parameter combinations could be useful and would be feasible but require a very detailed discussion on what is realistic and what is not. We would rather like to put this aspect on the to-do list for the future development and have added the following sentence to the conclusions chapter:

"Further, we intend to implement an intelligent warning function, informing users if they provide inappropriate combinations of parameter values."

Table 1: Might I suggest putting the parameter explanations below the relevant equations, or right next to it? Now its hard to read the table and go back and forth to the small table caption.

We understand the arguments of the reviewer. However, the table would become quite lengthy and there would be a lot of redundancies, because various parameters are used in more than one equation. In order to keep the table compact, we prefer to keep the explanations in the caption.

Table 1: Can the ambient drag coefficient be spatially distinct? Is there a parameter that can be used to depend on land cover variations for energy loss in fluid like flows (forest vs grassland for example?)

Yes, the ambient drag can be provided as a global value or as a raster map. Also, all other key parameters (frictions, etc.) can optionally be provided as raster maps. We have added the following statement in Section 2.2:

"All the key parameters used in the equations of Table 1 can be defined as global values or as raster maps, where individual values can be provided for each raster cell."

The fluid friction number (corresponding to the Manning number) accounts for the roughness of the basal surface in fluid-like flows. We have made this aspect more explicit in the revised caption of Table 1:

"n = Manning number, accounting for the roughness of the basal surface for the fluid phase;"

Line 140: I read this as meaning the phases are separated to form the individual layers? But that seems to come with more complications mathematically for the momentum transfer between these layers. Can individual layers be multi-phase? Now the term layer and phase are mixed and were a bit confusing to me.

Individual layers cannot be multi-phase, each layer represents one phase and momentum transfer between layers indeed represents an issue. Still, each layer can be used as a mixture of different types of materials, with their specific material properties and rheologies lumped into one parameter set. We have extended the explanation in Section 2.3:

"In the present work, we simplify this pattern to a maximum of three layers, with phase 1 at the bottom, an optional phase 2 in the middle, and phase 3 at the top, i.e., the individual phases are arranged on top of each other instead of mixed together (Fig. 1a). Thereby, each layer represents one phase, meaning that individual layers cannot be multi-phase. "

Line 155 – 165: I am not completely against this approach here. Data collection and lab results would be prohibitively difficult, but currently this could use a rephrasing I think. Principle i) seems somewhat strange to me, layer surface elevation or layer absolute height? I sort of see a geometric explanation for why this increase with the angle of the interface would work to replace the general drag force normally used by the authors, but in my mind assuming the drag coefficient is 1 for a vertical interface breaks some of the original assumptions in the derivation of the drag coefficient in the earlier works of the authors.

Clearly, the drag in the multi-phase mass flow models (Pudasaini, 2012; Pudasaini and Mergili, 2019; Pudasaini, 2020) is the drag between the fluid and particles, which are mixed together. The drag in the layered model is the drag between the layers, mainly depending on the geometries of the interacting layers in contact. So, these two concepts of drag are fundamentally different and not connected to each other. While the drag models presented in Pudasaini (2012), Pudasaini and Mergili (2019), and Pudasaini (2020) are based on some fundamental principles, the drag between the layers is a kind of technical aspect that needs further work and amendments.

In regard to principle i), it is actually the thickness gradient of the layers which is used (this has been clarified in the revised manuscript). According to what was said above (drag in the layered model representing a completely different concept than in the model with mixed phases), we do not see a general implausibility in assuming a very high drag in case of a vertical (or slope-normal) interface. However, due to the grid-based nature of the model, such an interface is not possible.

2.5: How do the authors deal with the often observed influence of the increasing and decreasing pore pressures during the compression and spread of slow-moving landslides?

See response to the next comment.

It seems now the dynamic pore pressure is not a remaining contribution to the momentum equations? I do like the approach to accommodate more types of movements, but this limitation might be mentioned or explored?

Our intention was to provide a simple and intuitive approach, at the cost of neglecting or simplifying some aspects, such as the evolution of pore water pressure. We have clarified this important aspect as follows:

"It represents a simple and intuitive approach, at the cost of neglecting or simplifying some aspects, such as the evolution of pore water pressure."

2.6: It would be nice to see a figure here with an example already (even though its shown later in the manuscript)

We understand the argument of the reviewer. However, we think that it would disturb the structure of the paper, as results should not be shown in the methods section. Therefore, we prefer to keep Fig. 12 in the results chapter.

Table3: I find this table not so needed in the full story. I think the point is made well by the sentences above it.

We agree with this comment and have removed Table 3 from the revised manuscript.

4.1: To what extent is the discretization of the layers a major influence on the final output? Now these discrete layers move separately as units, but could you remark on the balance between the influence of the model assumptions/numerics here vs the physics of such a hypothetical slide?

When assuming a mixture of the three phases, the typical topography of such slow rock slides as it is observed in nature, with backscarps separated by small valleys, would not at all be reflected in the simulation result. Instead, there would be just one step in the surface, at the top of the landslide mass. We did not do a comparative simulation of such situation for the preprint, but we intend adding it in the revised manuscript, in order to clarify this issue. Finally, we do not show the comparative simulation in the revised manuscript as, in our opinion, it would not add a lot of value. However, we include it in this response document as Fig. R1.

Line 365: Very good visualization of the differences. I would perhaps want to ask the authors to clarify if they think the 2-layer approach would be the only valid one for such flows, or could, a single-layer approach with non-fragmented physics, potentially get equal behavior?

This is a very interesting point. In fact, the suitable approach depends on the characteristics of the movement. If it moves as one mass, with rather continuous deformation without a clear shear surface, the single layer approach would be most suitable. If there is a discrete shear surface in between a lower and an upper layer, the two-layer approach would be the best choice. In the case of the Köfels rock slide, it is not clear whether there was such a discrete shear surface. This is described in L264–269 of the preprint. Therefore, we test and compare both approaches.

Line 480: Would it be possible to detail somewhat more the numerical scheme in an appendix, as its mentioned several times, and provides some of the key observations in the application cases. Besides the citation from Tai et al., these types of models nearly always require particular details for their numerical solution. I do agree with the point, that diffusion is a necessary, and unfortunate by-product of these schemes. Although approaches with SPH are, as the authors say, perhaps needlessly complex. A potential direction could be a consideration of the non-linearity/non-smoothness of the terrain and layer data. Discontinuities of a landscape in flood models require particular attention in terms of hydrostatic reconstruction and flux limitation as to prevent diffusivity of the flows.

We use the numerical scheme described in detail by Wang et al. (2004). In our opinion, it is therefore not necessary to rewrite everything in an appendix. We have clarified this aspect in Section 2.1:

"We use the scheme in the way described by Wang et al. (2004)."

We fully agree that there is a lot of potential to refine the numerical implementation of the model. We consider this a potential future direction which is out of scope of this paper.

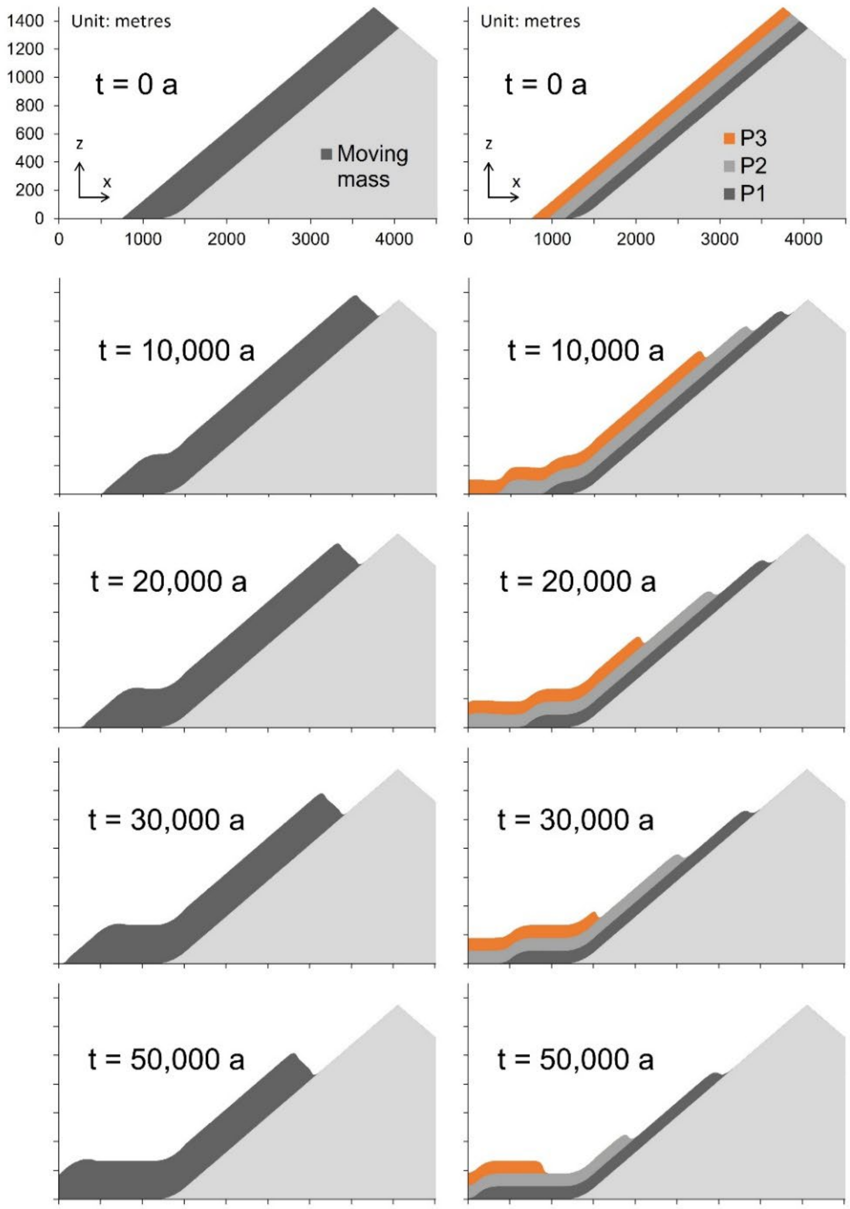

Fig. R1: Movement of the mass as one block (left side) and as three layers (right side).

Response to Reviewer 2

General comment:

The paper describes the new features of a landslide simulation software and presents numerical simulations highlighting the relevance of those new functionalities. The new features seem indeed substantial enough to justify a publication. The method and results are presented clearly and concisely. The description of the numerical experiments seems precise enough to allow reproduction.

The paper gives a rather clear description of the model, and of its new functionalities. The model is, for the most part, understandable without previous knowledge of the software. This is a very positive aspect, but some parts of the model description lack some clarity and could be improved (see next section).

The paper clearly motivates the need for the new features. The test cases are well chosen and the comparison between the different models is very well presented and easy to follow. The limitations of the model are also mentioned in the discussion, which is a good point.

After some minor modifications in the model description, I think the paper should be accepted for publication.

We would like to thank the reviewer for the careful reading of the manuscript and for the appreciation of our work. Please find below our responses in blue (where we agree with the comments) or orange (where we do not agree). Major changes in regard to the final response to the open discussion are written in purple colour.

Specific comments:

Section 1.

L27. The acronym GIS is defined nowhere. You could add what the acronym stands for.

We have defined the acronym at its first occurrence in the main text ("Geographic Information System").

L39-58. It is not clear whether the prototypes mentioned in the list (slow-flow process and deformation control) are the new features presented in the rest of the paper, and if there are already available in previous versions of avaflow.

For both models, prototypes were already available in r.avaflow v3. The case study on the Jiaokou tailings dam failure (Su et al., 2024) was using this prototype, which is very close to the model used in r.avaflow v4. Pudasaini and Mergili (2024a) specifically describe the prototype of the deformation control model – same as for the slow-flow model, also this component already existed in r.avaflow 3, as a prototype.

The introductory part, to which the reviewer refers, is meant to summarize the needs for extension of r.avaflow, not the additional functionalities actually implemented in r.avaflow v4. In the sections 2.4 and 2.5 of the preprint, it is clearly mentioned that the models used in r.avaflow 4 are based on the referenced prototypes, so we do not see a need to modify the text in this respect.

L52. If the computational experiments are published results, please add the reference.

They are no published results, just tests. We have added the term "preliminary" to the text in order to enhance clarity.

L55. What is GRASS GIS? A software?

Yes, GRASS GIS is an open-source GIS software. We have indicated this in the revised manuscript and added a reference.

The introduction is overall very clear and well structured. However, there is no reference or comparison to other mass flow simulation tools (e.g. Shaltop). Putting some context here would help to situate r.avaflow.

Yes, this is an important point. A detailed comparison with other software is out of scope here, but we have added a paragraph to the introduction, setting the context and mentioning other relevant tools:

"Mass flow simulation tools are used for numerically reconstructing, predicting, and communicating flow-type landslide processes. The first conceptual model for mass flows dates from the 1930s (Heim, 1932), and the first physically-based approach from the 1950s (Voellmy, 1955). More sophisticated models have evolved from the earlier models over time. By the mid-2020s, many practitioners still prefer physically rather simple models put into user-friendly interfaces, such as the RAMMS software (Christen et al., 2010). At the same time, scientific attention is paid to two-and multi-phase models such as OpenLISEM, integrating slope hydraulics, landslide release, landslide, and landslide runout (Bout et al., 2021) or r.avaflow, a flexible multi-phase computational framework focused on complex, cascading landslide processes, and on a full 3D implementation of the material point method (Cicoira et al., 2022). We focus on r.avaflow, …"

Section 2.

L75. Please write what the TVD-NOC acronym stands for.

We have added this information: TVD-NOC stands for "Total Variation Diminishing Non-Oscillatory Central Differencing" (e.g., Tai et al., 2002; Wang et al., 2004).

L76. The information that the grid is moved half a cell does not seem relevant here. Either add more details or remove the sentence.

We have removed that sentence.

L79. Please specify what does a hydrograph record in your context.

Hydrographs are defined by discharge. Flow velocities are also provided at those locations. This piece of information has been added to the revised manuscript.

L90. It is not clear whether the indices x,y,z denote derivation with respect to x, y, t, or if it is just the name of your variables.

The indices denote derivation in time, x direction, and y direction. The text below Eq. (1) has been slightly changed to enhance clarity.

L91. The flux are expressed as the derivatives of which wuantity? A word seems to be missing here.

The derivatives of the momentum, the formulation has been changed accordingly.

L91. I don't understand how you can neglect the time derivative since all your experiments are time-dependent. Or do you mean that you won't describe the term F_t in this section? For the sake of completeness, the term F_t should be included in Table 1.

Thanks for this comment, we expressed this aspect in a wrong way in the preprint. The time derivative is, of course, considered in the way described by Wang et al. (2004). It is just implemented in the code in a different way than Fx and Fy, therefore the confusion. We have added the time derivative to Table 1.

L108. I think it should be g_x^* instead of g_x.

Thanks for pointing out this mistake – corrected.

Table 1.

- For the fluxes in x and in y directions, please add parentheses to show clearly which terms are differentiated.

It is the entire terms which are differentiated, we have indicated that by square brackets.

- For the internal deformation in the fluid, the "+" sign is not necessary.

We think that it is necessary. The term in the square brackets is added to the term before, but only in those cases where buoyancy has to be applied – which is generally not the case in the layer mode. We will, however, check all equations in detail again before submitting the revised manuscript.

Table 2.

- The expression "mechanically controlled" is defined nowhere in the text. Unless it is a classical notion in the target community, please explain what it is.

The mechanically controlled deformation of landslides is based on the principle of material strength or resistance and includes the mechanisms of internal friction, cohesion, viscosity, and yield strength that act against the deformation induced by the free-surface or the hydraulic pressure gradient of the landslide (Pudasaini and Mergili, 2024). This controls the landslide deformation and, in turn, also the motion and run-out, and offers a unique possibility to describe the landslide motion ranging from a fully non-deformable body sliding along the mountain slope to a completely fluidized motion without any resistance against the force associated with the free-surface pressure gradient. We have extended the corresponding line in Table 2 as follows:

"Internal deformation of the moving mass is mechanically controlled, based on material strength or resistance, …"

For a more detailed explanation, which would make the model description too lengthy, we refer the readers to Pudasaini and Mergili (2024).

L144. If I understood correctly, the layered model is described by the governing momentum Equation (1) and Table 1. When you describe the controlled components (gravity, flow height, drag), you could refer to their notation, either in Equation (1) or in Table 1. It would help a lot to understand how the equation is actually used.

We have added the notations in the way they are used in Table 1.

L149, 150. Similar remark: the notation h_e appears nowhere in the equations before, so it is difficult to understand what role the effective flow height plays in your model.

The notation h_e is meant only to explain that the heights of the individual phases are used in the layer model instead of the total flow height. In our opinion, this is clearly explained in L149–151 of the preprint, so that no further explanation is needed.

L155. Please specify the value of C_{DX} when the conditions are not met.

The statement “If the condition given in Eq. (2) is not met, C_{Dx} is set to 0.” has been added to the revised manuscript.

L155, 156, Figure 1. The equation is written for the phases P1 and P2, but the Figure 1.(b) shows results for P1 and P3. It is not clear to me whether P2 necessarily represents the mixture layer, and if its properties are fundamentally different from P3.

This is a good point. In Eq. (2), P1 and P2 are just used as an example, the same principle applies also to combinations of P1 and P3 or P2 and P3. We have clarified this aspect in the revised manuscript.

L175. Same remark as before, the deformation factors f_{dx}, f_{dy} do not appear in the governing equations.

The governing equations are for x direction only, fd should be fdx in Table 1, this mistake has been corrected.

L176. If you have only one parameter f_d^* controlling both deformation factors, it means that you don't have complete freedom over the choice of those deformation factors. Does this choice of parametrization have a physical meaning? Could you also give practical cases, for example: which value should be given to f_d^* to completely lock the transformation?

This was a mistake in Table 1. The f_d in the extended Coulomb term should actually be E_v (corresponding to the table caption). This mistake has been corrected. $f_d^* = 0$ would reduce deformation to almost 0, except for numerical effects.

L182. The choice of vocabulary is a bit confusing: "deformation control" and "sliding model" refer to the same functionality, but seem to describe very different physical behaviors.

In fact, “sliding model” would refer to the limiting case of controlled deformation. With fully locked deformation ($f_d^* \sim 0$), the mass is just sliding, not flowing. With $f_d^* \sim 0.5$, there is a combination of flowing and sliding. We agree with the reviewer’s concern and have replaced “sliding model” with “model with deformation control” throughout the entire manuscript.

L182. Please be more precise: which combination are not possible, and why?

We were referring to the thought that combining phases with different deformation behaviour in a mixture could be problematic. After rethinking the issue, we have removed this statement as it might be not completely true – some more testing will required which is, however, out of scope here.

L184. The gravitational acceleration of the entire landslide seems to be a new functionality that has not been introduced earlier in the paper. What motivates this new feature? Is this case still described by the momentum equation (1)? Again, the parameters g_x, g_y, g_x^g, g_y^g, f_g do not appear in the equations before.

We agree that the purpose of this approach needs to be explained in more detail and have reformulated the introductory sentence to the paragraph:

“In cases where landslides move as one rigid block, it might be reasonable to assume that the motion of the entire block is governed by global values of the gravity components, applied to all of the raster cells instead of the local gravity components depending on the local slope components.”

g_x, g_y, and g_z are the local gravity components (at each raster cell) in each direction. This is indicated in the text of the revised manuscript. In our opinion, g_x^g, g_y^g, g_z^g, and f_g are sufficiently (though briefly) explained. A more detailed description would become too technical and, in our opinion, make the section quite lengthy.

L192. Since you are giving the order of magnitude for the velocity of slow landslides, you could also give it for extremely rapid flows.

Thanks for pointing this out – there is a mistake in this sentence. The definition of slow landslides differs from the definition in the Hungr et al. (2014) paper. We have amended the introductory part of the paragraph as follows, in order to enhance clarity:

"Hungr et al. (2014) describe all landslides moving at >5 m s^{-1} as extremely rapid. This velocity class roughly defines the scope of the momentum balance model used in r.avaflow. In order to extend the applicability to slower landslides, an equilibrium-of-motion model has been adopted."

L200. There is a notation conflict, since F is already the flux in Equation (1).

Thanks for pointing this out, we have changed the variable name of the force acting on the landslide mass to F_L.

L213,214. The viscosity term and the ratio between kinematic viscosity and the square of the basal layer are denoted by the same letter \xi.

Thanks for noting this. The statement ", and ξ is the viscosity term for the basal layer" is incorrect and has been deleted in the revised manuscript.

L215. Which equations are you finally solving? The model described in Section 2.5 seems very different from the previous sections, so I assume you are not using the Equation (1). Can you write the governing equation? Can the slow-flow model be combined with the 3-phase model or with other new features from r.avaflow?

Yes, Eq. (1) cannot be used for the slow-flow model. Momenta for each time step are computed in Eq. (5), and mass and momenta are redistributed through the flux terms. Flux terms are the same as those used in Eq. (1).

Section 3.

As a general comment, I found the description of the test cases very clear and well written.

Thank you very much for this comment.

Figure 2.

- I don't understand why the two other layers are not visible in this view. Could you choose another point of view or add a figure to make all layers visible?

This is mainly an issue of colouring – we have changed the colours in order to enhance clarity and to show all three layers.

- The colors are very dark, and it makes this figure difficult to read (in color, and even more when printed in black and white)

Yes, we agree – see response to the above comment.

- If possible, please add the axes in the figure

We have added the axes to the revised Figure 2.

L278. Is there a motivation to take two layers in this test case, apart from testing the new feature? Does each layer represent a different type of rock, for example?

Yes, there is a hypothesis that the rock mass split into two parts, an upper and a lower one. This hypothesis which is, however, controversial, is described in L267–269 of the preprint.

Figure 6.

- Is this figure an output from the software? Please add the scales like you did in Figure 3(d) or 4(c) and the axes.

Yes, the figure shows the simulation output (which, however, has been post-processed). The figure has been revised, axes and scales have been added to the first pane.

Table 4.

- For SD1, please specify which values correspond to P1, P2, P3.

We have added an additional column to the table, depicting the phases.

Section 4.

Overall, the figures of this section look good and are very readable.

Thank you very much for your appreciation.

A general comment for the Section 4.3: the notations FO1, FO2, FO3 and FOC1, FOC2 are quite confusing. Can you use another notation for the control points?

Yes sure – as this is the only test case where we use control points, we have renamed them to C1 and C2.

L384. "Displacement wave" is not very precise, replace by "water wave"

“Displacement wave” is a commonly used term when referring to landslide impact into water bodies, we would therefore prefer to keep it as it is.

L390. "only slightly decreasing thereafter": did you mean decreasing or increasing? The curve does both.

Thanks for looking so carefully. Indeed, “decreasing” is incorrect, it is a very slight increase, but in fact the level remains more or less stable. We have changed the formulation as follows:

“Fig. 11 also displays this effect very clearly for the simulation FO1, showing a decrease of the sea level by maximum approx. 980 m for point FOC1 (Fig. 4), not recovering beyond a level of -900 m until the end of the simulation.”

L398. It took me some time to understand that you show the results of two different simulations on each profile (the dry simulation and the fluid and solid simulation). This is indeed mentioned in the end of Section 3.3, but you should recall it in the description of Figure 9.

In order to enhance clarity, we have added the following statement to the caption of Fig. 9:

“Note that the landslide deposit neglecting the interaction with the ocean is included in all profiles, referred to as “Dry”.”

L400. I agree that the landslide becomes less mobile for FO1, but for FO2 the landslide profiles almost overlap, how is it less mobile?

It is also less mobile for FO2, but to a much lesser extent – this aspect has been clarified by adding "… to a much lesser extent, also with …" to the text. A remaining issue, limiting the comparability of FO1 and FO2, are the different drag concepts, discussed in L471–476 of the preprint.

The figure 10 is very nice, but is described nowhere in the text. You could maybe put it in Appendix.

In L385, it is referred to Fig. 9–Fig. 11 – this includes Fig. 10, which we consider an important illustration. We would like to keep it in the main document and have rewritten the reference to "Fig. 9, Fig. 10, and Fig. 11", so that also Fig. 10 is clearly and directly referenced.

Technical corrections:

L95. "This implies that in sum the terms do not induce...": the formulation sounds weird. Add a comma before and after "in sum", or rephrase.

Commas added.

Table 1. When printed in black and white, it is almost impossible to tell the difference between the blue and the green letters. Please consider using colors with a stronger contrast (also for color blindness).

We have changed green to some bright orange. This should enhance both accessibility and readability in grayscales.

L135. "The way how gradients are computed,": I don't think "how" is necessary here, please check. Also, there should be no comma after "computed".

Yes, we agree – corrected.

L136,137. The whole sentence is clumsy, please reformulate it.

We agree and have reformulated the sentence as follows:

"In situations where the solid fraction is significant, the original water surface might not recover, depending on the specific situation and model parameterization. Such a behaviour is not realistic in those cases where a landslide deposits at the bottom of a reservoir."

L200. Please separate more clearly the two equations, for example with \quad.

Done.

Figure 3. "View direction in A" and "Profile in D": Replace "A" and "D" by "(a)" and "(d)".

We have changed the figure accordingly.

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