Response to Reviewer #1: MS eSurf-2022-31

Reviewer's comments are denoted by C and our responses are denoted by R, respectively. In the marked-up manuscript, the removed texts are in red and the edited/added texts are in blue color. I hope that the revised manuscript will be suitable for publication in eSurf.

General comments:

C: In this manuscript, exact solutions to a recent model by Pudasaini & Krautblatter (2022) are presented. The model is a simplification of the widely used shallow-water approach for modeling shallow granular flows such as avalanches and landslides. The advance in this manuscript is to extend the range of exact solutions computed by Pudasaini & Krautblatter (2022) to decelerating flows, allowing a wider range of solutions to be calculated.

R: I very much appreciate the Reviewer's time and interest in my work. My sincere thanks to the Reviewer for the comments and suggestions that resulted in the improved manuscript in which I appropriately addressed all the concerns raised as far as possible which are relevant to the scope of the present work. However, the equation system developed in Pudasaini & Krautblatter (2022) is not a simplification of the shallow-water equation but it is much wider than that in modelling the motion of complex frictional debris material involving a broad physics and mechanics of motion. Our equation includes granular friction, extensional and compactional behavior of the landslide mass, fluid induced lubrication and liquefaction, buoyancy effect, as well as the quadratic viscous drag, modelling a mixture which are not in the reference the reviewer is mentioning (see, Pudasaini & Krautblatter, 2022, Page 3; or Page 2-3 of the present manuscript). This is clear from the manuscript (ms) where the strong physics carried by the model has been explained.

C: The model presented in this paper is considerably simpler than the shallow water models commonly used in both research and operational contexts for avalanche prediction and mitigation. The advantage of this simplicity is that explicit exact solutions can be found for avalanches that are steady or spatially-uniform, and implicit exact solutions can be found for flows varying in space and time.

R: There are two important things that I like to mention. First, the model presented in this paper is much complex from its physics associated with the frictional term and the mechanical response, and thus wider than the classical shallow water equation, please look on the forcing terms in the model equation (1) for the mechanical strength of the considered model. Second, shallow-water-type models are usually used for the computations. However, here I have presented the first simple analytical model and its particular and general analytical solutions for the motion of the landslide mixture down the slope.

C: While I am generally supportive of the approach of mathematical analysis of simple models, I have three major concerns about this work, which in my view mean that it is not suitable for publication in ESurf:

R: I value and I appreciate the reviewer's general support of the approach of mathematical analysis of simple model. However, the reviewer's comments contradict this aspect. But, as reflected by the comments the reviewer's view/perception on the relevant physical, mechanical and dynamical aspects of landslide appear to be not wide enough/weak. The reviewer fails to understand the power of exact analytical solution. This I explain below.

To date, there exists no exact analytical solution to the landslide motion as I have seminally presented here which varies as function of space and time for both the accelerating and decelerating motions down the entire slope. While I have improved and strengthen the ms as far as possible and necessary by addressing the legitimate concerns of the reviewer, several comments, however, are either not applicable, or not within the scope of this ms. Many of the reviewer's comments are irrelevant to the present ms. Some claims are even incorrect and inappropriate. The following statements make it very clear that this ms fits well to the scope of esurf.

In this ms, I constructed several beautiful, complete and unified, useful analytical landslide velocity solutions with pleasing first-hand notations and derivations. Since the Burgers' (1948) and Vollemy's (1955) restricted analytical solutions (which have immensely impacted the science), after more than 65 years, for the first time, I rigorously and very carefully derived many general, complete analytical solutions of an extended model that I generated for which Vollemy's and Burgers' solutions are only special cases. I very carefully wrote this ms. Technically, the results presented in different figures demonstrate that, computationally costly simulations can now be replaced by a simple highly cost-effective, clean and honourable analytical solutions (almost without any cost). This is a great advantage as it provides immediate and very easy solution to the complex landslide motion. So, I have presented a seminal analytical technique describing the entire landslide motion and deposition process. I was hoping that, as the previous eSurf paper (https://doi.org/10.5194/esurf-10-165-2022, Pudasaini & Krautblatter, 2022), this ms would be adequately respected by the reviewer as the new ms has much greater value than the published one. So, in my opinion, the scientific essence and novelty of the new solutions are enormous that broadly impact the science. I thought, as in the previous eSurf contribution (https://doi.org/10.5194/esurf-10-165-2022) the reviewer would recognize these inventions. Reviewer could already find answers to many of the questions raised by the reviewer in the submitted paper. I feel that there are conflicts of interests as the reviewer unjustifiably turned negative, unnecessarily causing troubles. In my opinion, such review is not compatible with the philosophy of the professional-community-based, open-interactive-journal like the eSurf. However, I have adequately improved the ms by considering necessary comments into account to enhance the ms.

C1: My primary concern is that the simplifications that have been made in the underlying model are so great, that the model is simply not relevant to real earth surface dynamics. I believe the model needs validation, including comparison of its predictions against the well-established shallow-water models and/or observations.

R1: Models are always based on some simplification of the very complex natural phenomena. Even so is the need to further simplify the more general model such that advanced analytical solutions can be constructed. This is the main essence here. It is quite clear that this ms is not concerned about the numerical solution, this is irrelevant here, rather the aim is to present the first-ever general exact analytical solutions with advanced mathematical methods. The results presented in this ms are relevant to describe the earth surface process, the landslide motion. This is evident from our very recently published research works in eSurf (Pudasaini & Krautblatter, 2022) on which this ms is developed.

What the reviewer talks about the shallow-water solutions, the reviewer is talking about the numerical solutions, but to date, there exists no exact analytical solution to the avalanche motion as I have presented here. It would be nice, but not all fundamentally novel exact analytical solutions must be validated right away at the time of constructing the solutions. It is the question of time and will, soon or later researcher may use it for various purposes. This has been proven with many of our previous analytical mass flow model equations, which become leading contributions in the field (see, e.g., https://doi.org/10.1029/2011JF002186; https://doi.org/10.1029/2019JF005204). So, instead of pushing the new good science forward the reviewer is pushing it back. Details on these aspects are explained below.

C2: The paper appears to be aimed at practitioners who would benefit from a simple formula for avalanche velocity, but various aspects of the solutions presented appear make them unsuitable for this task.

R2: The main aim of this ms is rather to present general exact analytical solutions to the landslide motion. Practitioner's benefit is the application aspect as the solutions can be utilized by any physical scientist and engineer. However, it is not quite clear what the reviewer is talking about why the exact analytical solutions presented here are “unsuitable”. Such implicit statements are not supported, because, I have, in fact, presented the first-ever simple and complete general exact, analytical solutions for the avalanche motions, and have explicitly mentioned/discussed with examples in several figures on how the mountain engineers and practitioners can use these solutions in solving applied problems that was not possible by any existing analytical solutions as the previous solutions are either applicable only to time, or only to spatial variation of the motion down the slope, but not including the variation of both the time and space which is exactly what is needed in real applications.

3. Some of the results presented are not solutions of the governing equation, due to the exact solution method used being used beyond its point of validity.

R3: These claims are not following the spirit of the ms. First, the results are presented to the simple governing model equation that unifies the mass and momentum balance equations (equation (1)). Second, as made it very clear in Pudasaini and Krautblatter (2022), and also in this ms, there are both the mathematical and natural aspects of the presented solution methods. However, if for some mathematical reasons, someone just wants to restrict the solution domain before the solution becomes multi-valued, there is no problem in doing so. Otherwise, if some known mathematical tools do not satisfy some natural phenomena does not necessarily mean that we should not think about seeking to cover a wider natural spectrum than to be too ceremonious. This will be discussed in more detail below.

Details of these points are given below:

C1. The model is not a realistic description of natural avalanches and landslides

R1: As explained below and in the ms, as with any model, within its scope, the model considered here is a realistic description of avalanches and landslides. So, the utility of this claim is weak.

C(a): The model used in this paper is independent of avalanche thickness. This means that the model predicts a landslide runout distance that is independent of the volume of material in the avalanche. This contradicts possibly the most fundamental observation of natural avalanches and landslides, namely that the runout distance (and avalanche velocity) increase with increasing avalanche volume. Consequently, I have serious concerns as to whether the work in this manuscript is of relevance to real avalanches or landslides.

R(a): Avalanche thickness (and its gradient) is not of concern here that I am working on this in a separate ms. Even without avalanche thickness, present exact analytical solutions can be used to solve many technical problems as the new solutions are far better than the widely used Voellmy and Burger’s solutions. This has been exclusively discussed in Pudasaini and Krautblatter (2022) and also in the present ms. However, the reviewer ignores this important aspect and even questions this fact.

C(b): Gradients of the thickness of the avalanche enter into the model equations through the expression for alpha on line 95. However (though it is not stated explicitly) alpha is then assumed to be constant (or a prescribed piecewise-constant function). This is a very significant assumption that is not justified in either this manuscript or in Pudasaini & Krautblatter (2022). It differs significantly the dominant 'shallow-water' modeling approach where conservation of mass and momentum are used to determine how the thickness varies as a function of space and time.

R(b): We know well about the importance of varying velocity and flow depth as we have developed several widely used sophisticated multi-phase mass flow models. However, again here, the reviewer is talking about the numerical simulation of the physically simplest hydraulic pressure gradient driven shallow-water equation, but neglects the essence of this ms in constructing the exact analytical solution of the model equation which assumes negligibly varying flow depth. Based on the underlying physical parameters from the field and laboratory data, the values of collective model parameters alpha and beta are carefully estimated and justified in several previous contributions whose values are used here with references, and are properly chosen (please see Lines 299-309; 410-423 of the marked up ms).

C(c): Surprisingly, neither this manuscript nor Pudasaini & Krautblatter (2022) attempt to validate the new model. To validate the assumption of (piecewise) constant alpha, I would like to see comparison of numerical solutions of a shallow-water type model (e.g. equations 1 and 2 of Pudasaini & Krautblatter 2022) with the corresponding velocity equation (equation 5 of Pudasaini & Krautblatter 2022). The commonly-studied initial conditions of a 'dam-break' release of a finite mass of material could be used as one text case. Good agreement between the velocity fields and runout lengths of the two models would provide some reassurance that the assumption of constant alpha is reasonable.

R(c): Here, the reviewer is asking avoidable question. Equation (5) in Pudasaini and Krautblatter (2022) is developed by combining (1) and (2) therein to develop a single equation such that exact analytical solution can be constructed for the most important dynamical quantity explaining the landslide motion, the landslide velocity. Given the aim of this and the previous paper, contrary to what the reviewer says, there is no surprise. The model (5) is not built to validate (1) and (2). Reviewer's question in not line with the aim of this ms. Even the reviewer does not seem to understand the essence of the model (5) in Pudasaini and Krautblatter (2022). Similarly, as clearly mentioned in the ms, validation is not the focus here, that can be done later as the presented new results, in my opinion, are sufficient to constitute a good new paper. Moreover, dam break problem is not relevant here, reviewer's comment is strange. Alpha is a collective model parameter exclusively based on the physics of the involved material. So, any value of alpha thus constructed is fully justified physically. Yet, new solution may be constructed with further development of the present model in which alpha may be considered variable. This can be possible, but not within the focus here. Moreover, verifying and validating the numerical solutions is another important aspect of the exact analytical solution that has been explicitly mentioned in the ms.

Surprisingly, the reviewer even attempted to question the legitimacy of the important results recently published in eSurf (https://doi.org/10.5194/esurf-10-165-2022, Pudasaini & Krautblatter, 2022) on which this ms is based. This cannot be appreciated. Rather than focusing on the novel and important aspects of the submitted ms and appreciating the new results, the reviewer is emphasizing on unnecessary things probably diverting the attention from the major contributions to irrelevant things.

C(d): The origin of the term -beta u^2 in equation (1) is unclear. It is described as a viscous drag coefficient, but is not of the correct form for either a Newtonian viscosity, nor a Chezy or Voellmy drag (in the latter cases I would expect a term scaling with u^2/h, introducing the volume dependence mentioned in point 1(a)). What is the physical derivation of this term? Is it validated by any field or laboratory observations?

R(d): The viscous drag term has clear physical origin that has been explained in many of our previous publications (as referred in the present ms), please see equations (1)-(4) in Pudasaini and Krautblatter (2022). The viscous term does not explicitly include 1/h. It has the form as in general use (Chow, 1959). Not everything should be explained in greater details in the new ms, this is the general practice in research for why we referred previous papers (see, Pudasaini and Krautblatter, 2022, and the references therein). So, this is an unnecessary comment.

C2. Setting aside the realism of the model, the exact solutions presented in this paper provide very limited value to the practitioners at which this paper appears to be aimed

R2: I am not setting the realism aside, rather trying to include the reality as much as possible with the model and the analytical solutions by proving the wider analytical solution than we have at present with my inventive method. As explained above and made very clear in the ms, I have, for the first time, presented the most general exact analytical solution to the avalanche motion that includes both the time and spatial variation. In this light, such solutions should be viewed properly. This remarkably advances our present understanding of analytical description of the landslide motion. The new solutions presented here bring significant scientific value with ample possibilities in application. However, demanding to explain everything at once with any exact analytical solution is not following the state of our knowledge, and not appropriately appreciating the important new science brought by the new model and analytical solution.

Here, the reviewer is forgetting and undermining the main essence of the exact analytical solution. It is a known fact that one should be very careful while talking about the exact analytical solutions of the underlying model equations. I have made it very clear in the ms, that the exact analytical solutions can only be developed by applying some assumptions in the main model equations. One cannot simply demand to construct analytical solution to the full model equation. This is not an acceptable comment. Still, the obtained exact analytical solutions to the simplified model equation can solve many practical problems. This is evident: as have been rigorously proven here, my exact analytical solutions cover both the simple analytical solutions of the well-known Voellmy and Burger's-type as spatial cases. Moreover, the mew solutions can be applied to much wider problems than Voellmy and Burger's-type appearing in natural and technological problems. These important aspects have been clearly discussed is the ms. However, the reviewer could not see these novel scientific values.

C(a): The model used in this paper is not predictive of avalanche thickness. This is an absolutely fundamental problem for using this model to assess landslide hazard (e.g. design protective structures, line 33), because the momentum and kinetic energy of a flow are proportional to the flow thickness.

R(a): I was expecting focusing on the merit of the ms rather than talking on what is not aimed in the ms. We know quite well the value of variation of both the avalanche velocity and thickness as the state variables. We advance step-by-step. As stated above, this ms does not focus on the variation of the avalanche thickness, and computing numerical simulations as the reviewer is irrelevantly asking. But, even considering the variation of the velocity alone, for the first-time, I have analytically constructed the most general exact analytical solutions to describe the motion of an avalanche down the entire slope. There are several important aspects I thought the reviewer would have considered. First, these solutions are much wider and physically far better than any existing analytical solutions for the landslide velocity that can be applied to solve several technical problems associated with the landslide velocity which would not have been possible before. In smoothly varying slopes, except in the vicinity of the inception, close to deposition, and also in the close proximity of the defense structure, the assumption of the constant depth of avalanche can be an acceptable approximation. Then the impact pressure (on protective structure) is calculated in terms of the velocity square. Second, one could work to further include the thickness variation in a separate ms.

C(b): In the simple solutions presented in section 5.1, the parameters alpha and beta are given various constant values. How are these values chosen? (e.g. in line 263, what is the process to 'properly choose' the parameters? In particular, how is the free surface gradient chosen, given that it is spatially varying and can take any value?) The solutions of the model are clearly sensitive to the values of alpha and beta. If the model were to be applied to a real avalanche, how could the value of alpha and beta be found (as a function of distance downslope)?

R(b): As mentioned earlier, based on the underlying physical parameters from the field and laboratory data, the values of collective model parameters alpha and beta are carefully estimated in several previous contributions whose values are used here with references, and are properly chosen. One can take any suitable constant value of the free-surface gradient, that is already included in the collective parameter alpha. As made it clear in the ms, the values of alpha and beta can be made technically varying spatially by choosing their different values in different sectors of the landslide track by carefully and suitably dividing it into several sectors. Some physically explained admissible values of the model parameters alpha and beta have been presented in the revised ms (Lines 299-309; 410-423). Based on this, I have presented several figures exclusively demonstrating how to do this. But, the reviewer makes unnecessary comments about this. One can work on the parameter sensitivity analysis later. However, for now, sensitivity analysis is not a major part of the fundamentally new, important analytical solutions presented here. In this ms, I do not want to add several more pages for this. Here, I aimed to present the broad picture on the dynamics of the newly constructed exact analytical solutions and their applications by displaying several figures for possible natural scenarios based on the legitimately constructed values of the collective model parameters alpha and beta that are exclusively based on the physics and dynamics of the flow.

C(c): The model solutions in section 4 are not explicit: they rely on numerical solution of implicit algebraic equations (19) and (22). As such, the model equations presented here require numerical solution (and potentially, identification of multiple solutions), and therefore do not have the advantage of simplicity associated with explicit exact solutions.

R(c): Unlike the false claim by the reviewer, the wide and general solutions in section 4 are fully analytical. Any statement by the reviewer around this point is absolutely wrong, and the reviewer lacks the proper understanding of the model and the analytical solution method presented here. I have constituted a mathematical theorem to formally and fully analytically construct the exact solution to the model equation (see, Theorem 5.1, Pudasaini and Krautblatter, 2022). I thought the reviewer would have been careful in making such clearly wrong statement.

R(d): The introduction discusses the value of computing exact solutions to equations, and I am in agreement that exact solutions are have significant value for assessing models and numerical methods. However, the problems associated with solving full shallow-water models numerically (line 50) are overstated in this paper. Numerical methods for hyperbolic systems, such as that of Kurganov and Petrova https://doi.org/cms/1175797625, are robust and well validated, and have become very widely used. Importantly, they are not computationally expensive, and can find accurate numerical approximations to one-dimensional problems, such as those studied in this manuscript, within a few seconds. Numerical shallow-water calculations of this sort have been the primary tool used in operational avalanche hazard mapping for some years. The numerical shallow-water approach avoids many of the shortcomings of the present manuscript, in that it can predict both avalanche thickness and velocity, using a realistic rheology, and can be applied directly to real digital elevation model topography.

C(d): I do not mean that the analytical solution presented here solve the full shallow-water-type equation. However, within the scope of the novel exact analytical solutions presented here, these solutions can be applied to check the validity of any numerical solution method to test its applicability including those mentioned by the reviewer. At least, if the numerical simulation method can produce closer solution to the presented solution here, the most possible exact solution, then such numerical simulation method can be thought better than what we know before. Definitely, analytical solutions are the fastest and the most trustworthy solutions that exists; we cannot deny this. We have a long time experience on solving the full dynamical model equations including mass and momentum balance equations with robust, high-resolution advanced leading computational tools based on GIS (r.avaflow: https://www.avaflow.org), even for the multi-phase flows with very realistic rheologies down complex terrains. Here, I am not that much concerned to the numerical solution of the shallow-water equation, rather with present the most advanced general exact analytical solution. Here, the reviewer appears to lose the aim of this ms.

About the reference provided by the reviewer: I am not aware of the use of the mentioned reference in practical application of frictional mass flows including debris flows, avalanches and landslides, because the said model therein is not for landslide, but for simple water flow, which cannot describe the complex frictional mass flows. So, I am wondering about the reviewer's statements.

Unlike the lengthy numerical simulation processes, the construction of fundamentally novel mechanical model equations and their completely new rigorous exact analytical, beautiful solutions are challenging and appear in literature only very seldom. In peer scientific review, I thought, such models and solutions are adequately valued. This is crucial. However, the reviewer could not see these important seminal aspects. One must be very careful in unnecessarily commenting on the exact analytical solutions. Numerical simulations cannot replace the exact analytical solutions, both in its rigor, foundation and speed. The energy we need in creating the physics-based exact analytical solution can be incomparably higher than that needed for the numerical simulation. Unlike huge efforts in numerical computations, even a small-looking contribution with exact analytical solutions may have much greater, and lasting values than the numerical solutions, because exact solutions are the truth within the given circumstances. However, the reviewer fully neglected the importance of the exact analytical solutions over the often-questionable numerical simulations (Pudasaini and Krautblatter, 2022). As it is clear, I have also substantially contributed in simulating and validating complex frictional mass flows with multi-mechanical, multi-phase mass flow equations I developed in the past years, which become leading models and the advanced computational tools widely used worldwide (Pudasaini, 2012; Pudasaini and Mergili, 2019; Mergili and Pudasaini, 2021). I clearly know the essence of both the analytical and numerical solutions. However, surprisingly, the reviewer unnecessarily mixed the things up creating confusion. I have properly discussed the importance of the exact solution and also adequately mentioned the usefulness of the numerical simulations in the present ms (Lines 49-59) and also in the previous publication (Pudasaini and Krautblatter, 2022).

C3: As noted in Pudasaini & Krautblatter 2022, the implicit equations 18, 19, 21, 22, have multiple solutions, and these are interpreted in this manuscript as a `folding' wave. This is incorrect mathematics: the solutions to equations (18,19,21,22) cease to become solutions to the governing PDE (equation 1) at the point that multiplicity of solutions starts (ie. when the gradient du/dx diverges). The `folding' process described in section 5.2.2 and 5.2.3 is therefore simply a mathematical artifact that occurs when the particular implicit solution method used in this paper is pushed beyond its point of validity. Therefore, the analogies made in section 5.2.3 between the shape of plots in figure 8, and folding depositional behavior in avalanche deposits are not valid. As the author is no doubt aware, solutions to shock-forming PDEs (such as equation (1) of this paper) only exist up to the formation of a shock, and require an additional equation (a jump condition) to be integrated beyond this point.

R3: About the beautiful and astonishing folding solutions, I have made it very clear in the previous publication (referred in the ms) and also here. I know all these aspects well. From the possible multi-valued nature of the solutions, these solutions may have restrictions from the mathematical point of view. There are two points that I have made, and again want to make, very clear. First, something that is mathematically problematic does not necessarily mean that that does not happen in nature and we should not seek to advance. Second, if someone does not want to use the general solution beyond the single valued solution, it is up to them how to utilize, possibly with further mathematical complications and advancements, e.g., using jump conditions after the shock formation (Pudasaini and Hutter, 2007). This may also open further research possibilities and directions. But, for the first time, I presented the general exact solutions that can clearly represent natural phenomena. This is the reason we wrote in Pudasaini and Krautblatter (2022): “Although mathematically folding may refer to a singularity due to a multi-valued function, here we explain the folding dynamics as a phenomenon that can appear in nature.” So, the comments on the folding solutions are a bit dramatized. However, the revised ms includes (Lines 533-539): “Following Pudasaini and Krautblatter (2022) we mention- although mathematically folding may refer to a singularity due to a multi-valued function, here we explain the folding dynamics as a phenomenon that can appear in nature.” The text has been improved accordingly.