Earth Surf. Dynam. Discuss., referee comment RC3
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Comment on esurf-2021-81

Anonymous Referee #2

Referee comment on "The landslide velocity" by Shiva P. Pudasaini and Michael Krautblatter, Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-81-RC3, 2022

General comments

The paper by Pudasaini & Krautblatter presents a series of detailed analytical solutions for the calculation of the velocity of a landslide, by considering the depth-averaged forms of incompressible mass and momentum conservation equations for a mixture of solid materials and a liquid (treated as a single-phase flowing material) as a starting point. It is shown that the analytic solutions though they are simplifications of the reality can capture many crucial mechanisms, such as the longitudinal stretching (the model remains 2D) of the mass during its movement along the slope, wave breaking and landslide folding. Moreover, simplified versions of the new analytic equations (equations numbered 11, 19 and 39-40 in the manuscript) allow to retrieve other simplified existing models, such as the ones given by solving the center of mass model developed by Voellmy --initially for snow avalanches-- or the inviscid Burgers equations developed in fluid mechanics.

I found the scientific content of the paper very interesting with many interesting ideas discussed all along the development of these new analytic equations for the velocity of a landslide. Moreover I think that the paper is a niece piece of work for educational purposes, though I have some concerns regarding the presentation (see my general recommendation below).

The equations proposed by Pudasaini and Krautblatter remain a simplification of the reality but they take into account several key contributions to describe the motion of a landslide and thus are powerful tools to assess landslide risk and conduct relevant calculations for practitioners who are in charge of the hazard quantification and mitigation. They can complement more sophisticated calculations based on complicated numerical simulations, better than current analytic solutions based on center of mass models largely used in engineering (Voellmy-type models) can do.

As such, I have a very positive feeling on this work. However, I have a number of concerns that need to be addressed before I can definitely recommend it for publication.

The scientific content is very good and very interesting, overall. My concerns are on the presentation of the material. I must say that the paper was very difficult to digest (though I was excited to read it!). I really think the authors need to revise a lot the presentation to make the paper shorter and clearer.

Here are some suggestions for that purpose:

1) the introduction is very very long: it really needs to be revised and shortened. There are a number of arguments repeated. I don’t think that the authors need to oversell their results. They just need to present the facts and summarize them, this will be more efficient.

2) I would suggest the authors to change the organization of the paper by starting with a presentation of the new analytic solutions: firstly with section 3.2 (eq 11), secondly with section 4 (eq. 19) and thirdly with section 5 (eqs 39-40). The simpler solutions for steady-state motion (eqs. 6, 7, 8) can be shortly presented as specific cases, after eq 11 or even in a supplementary material. The manuscript needs to be shortened.

3) the summary section (section 7) is very long, with many repetitions again. And most of the material is already clear and said in the discussion section (section 6) or elsewhere in the paper. I think the summary section should be removed. If there are key ideas in the section 7 which the authors want to keep, they should be moved to the section 6 then and that’s it.

4) there many superlatives or sentences that intend to praise the work done by the authors. Please let the readers themselves appreciate the quality of your work! I think those superlatives or statements that oversell your work are not needed and should be removed: see my suggestions in the list of specific comments below.

In the following I give a list of more specific comments.

Specific comments

- l. 30-31 : in some (slow) flow regimes, the impact of a landslide may be more a consequence of its total mass (size/volume) than a consequence of its velocity. And the velocity is not the prevailing parameter to estimate the impact force in this case. I think the statement at l. 30-31 should be qualified or, even better, this last sentence of the abstract can be removed (the impact force is not addressed in the current paper).

- l. 135 : I don't understand \alpha_s = 1… I guess that we have : \rho_mixture = \alpha_s \rho_s + (1-\alpha_s) \rho_f. The dry landslide case thus corresponds to \gamma that tends toward 1 (\gamma is equal to an epsilon when \rho_f is the density of air and very small compared to the grain material density \rho_s but is never zero in fact in an "air environment") and \alpha_s is the volume fraction of the grains in air, which is restricted to the maximum random close packing (always smaller than 1) of the granular medium ? Please check/fix this.

- l. 155: "genuinely" should be removed.

- I'm not sure that section 2.3 is needed: there are many arguments presented here that are repeated all along the manuscript and in the section 6 (and 7) again. This section may be removed to shorten the manuscript/avoid repetitions. The main lines could be added after the key eq 5 (end of previous section 2.2) and that's it.

- l. 179: "… superior over..." I don't know if this a good statement to keep. This could be replaced by "… prior to...". I think the most important is to understand the foundation of the physical equations and make "them talk" by considering asymptotic solutions (first step) before launching sophisticated simulations based on those physical equations that are solved by complicated numerical schemes (second step). Sometime modelers forget the first step which is very useful to interpret complicated simulations and/or avoid mistakes in the second step. I would say for instance: "… and are often needed as a prerequisite before running numerical simulations based on complicated numerical schemes (yet based on the same physics in the end)."

- l. 191-192: eq 6 reduces to the center of mass model for a dry landslide if \gamma =0 (or \rho_f is very small compared to \rho_s). Maybe this should be specified here.

- figure 3: the 2 plots are already presented in figures 1 and 2. Either you remove figure 3 or I wonder whether you could consider instead some normalized versions of the plots: velocity/Uo versus x/Lo (top plot) and velocity/Uo versus t/To (bottom plot), where Uo=sqrt(alpha/gamma), Lo=1500m, and To=Lo/Uo.

- l. 273: "This is a fantastic situation." can be removed.

- l. 299: a strong shaking is an example of an initial input of strong kinetic energy but other situations are possible when a high potential energy is available and is converted quasi-instantaneously into kinetic energy (e.g. when the vertical drop of the detachment area is huge and combined with a high slope angle of the terrain).

- l. 334: “unique” can be removed or replaced by another word (like “interesting”) to tone down the statement.

- l. 336 and 339: I’m note sure that “maximum” is adequate here. Why not using something like U_s-s or U_infinity for the steady-state value ?

- l. 344-354: this is a classical discussion, already addressed for the classical center of mass models. Please note that this discussion could be extended/updated by considering more recent approaches on center of mass models which considered some prescribed shapes for the profiles of the terrain, such as cycloidal and parabolic tracks (e.g. see Gauer CRST 2018).

- l. 461-462: regardless of the model discussed, either the classical center of mass models or your model proposed here, I think that such models give upper bounds because they are not considering (by nature) the lateral spreading of the mass of the mixture when coming to rest. Such 3D (or 2D lateral spreading) effects when they are considered should give lower velocities and run-out. Please be careful and keep in mind that your more realistic model (I agree) can also give overestimates then. This is the reason why full 3D numerical simulations on a digital terrain model are needed too.

- l. 530: why choosing the powers 0.5 or 0.65 ? Is it fully arbitrary or do you have some arguments?

- l. 551-552: I agree that the initial conditions influence the dynamics over space / time but in the end the asymptotic states for sufficiently long distance (long times) are the same when the landslide come to a standstill, as shown in the two plots in figure 10. On other words, the way to go towards Uo=sqrt(\alpha/\gamma) is not the same but Uo is reached at the end of the day. Could you comment on this ?

- l. 585-586: “This is a fantastic situation.”, again, can be removed !

- l. 630: “This is a seminal understanding” is not needed I think. No need to oversell your work. The reader can appreciated its scientific quality by themselves.

- l. 691-692: the last sentence of the paragraph should be removed.

- I would suggest to remove the section 7 which repeats many statements already given either in the discussion section or along the main text of the manuscript. The manuscript is very long, difficult to digest. Section 7 is not needed.

Technical corrections / editing typos

- l. 209: replace “has been” by “this will be”.

- l. 238-239: “… description. Both...” Please revise, should be one sentence.

- l. 318: This section starts by “Second… ”, the first point being in previous sections (l. 312). This needs to be fixed. The two points should be in the same section. Please check and revise.

- l. 387, l. 391 : note sure that “ms-2” is a correct notation; should be replaced by “m s-2” (empty space) or “m.s-2” (dot)?

- l. 421-422: “So… identify” can be removed; already said on l. 419 just above.

- l. 447: replace “Section 4.5” by “the current section” or “this section”.

- l. 555: replace “has been” by “is”.

- l. 710: “large” repeated twice; please fix.