Dear Reviewers, dear Editor,

thank you for your comments, although (as nowadays common in geosciences) mostly referring to the description of the state of the art. The points addressed in the reports are discussed below, where changes to the manuscript are highlighted in bold letters. Line numbers refer to the version with highlighted changes.

Best regards,

Stefan Hergarten

Reviewer 1

Paper is definitely interesting, but for me (I'm geologist) it was difficult to follow the explanation.

The theory may indeed be quite tough for "average" geologists. This is why I used a somehow unusual structure (not "Methods", "Results", ...) with two sections (2 and 3) developing the new ideas and already illustrating them with examples. Section 4 becomes more difficult since combining process parameters to length scales (λ) or to nondimensional properties (energy ratio ϵ) is not widely done in geology. The consequence is that readers who want to understand why the modified rheology reproduces the scaling of $\frac{H}{L}$ with volume quite well have to go through it step by step.

It would be really useful if you will add a list of all parameters used in the equations with all these quantities dimensions and their physical meaning (may be a schematic cross-section). It will help a lot to readers from the geological community to follow your explanations.

I added a schematic cross section (new Fig. 2) that illustrates the geometric properties (H, l, β, h, S, L). This will probably help to keep track of the variables. **I also added some more explanations (lines 122–123, 133–139, 162–166, 251–252, 302–303).**

Some comments are in the attached file.

Line 11: Generally, long runout is not a characteristic feature of rock avalanches (see RA definition in Hungr O, Leroueil S, Picarelli L (2014) Varnes classification of landslide types, an update. Landslides 11:167194).

Sorry, but I have no idea what to do with this information even after scanning each occurrence of the word "avalanche" again in the aforementioned paper. The highlighted sentence about the height-dominated regime is one of the fundamental findings made here and thus cannot be related to the aforementioned paper.

Line 23: L refers to an axial line in plan view (see Li et al., 2022 DOI 10.1007/s10346-021-01828-w).

The approach proposed by Li et al. (2022) for characterizing the geometry makes sense. As far as I know, however, it has typically been measured (in particular, in the past) as a straight horizontal line.

Line 25: I do not think that it is correct – during RA emplacement intensive fragmentation evolves and debris spreading might be impossible if not considering it.

It looks a bit to me as if you want to bring fragmentation into play. I am aware that some researchers are promoting this concept very actively. To my knowledge, however, is has only been shown that fragmentation can in principle increase fluctuations in velocity, while it obviously consumes energy in total. I am not aware of any studies that were able to predict the decrease in $\frac{H}{L}$ with volume quantitatively based on fragmentation. As soon as anyone has succeeded in this, we can start the discussion whether my approach with the two flow regimes or fragmentation is better. Until then, however, just claiming that long runout cannot be explained without fragmentation (what some researchers are doing) is not a serious argument for me. Anyway, it is only stated at this point in the that either the runout of the center of mass must increase or there must be longitudinal spreading (regardless of whether this is related to fragmentation or not), and this should be correct.

Line 50: Turbulence might be correct for snow avalanches, but is not observed in RA (see, e.g. Strom A (2021) Rock avalanches: basic characteristics and classification criteria. In: Vilmek V, Wang F, Strom A, Sassa K, Bobrowsky PT, Takara K (Eds) Understanding and reducing landslide disaster risk: Volume 5 Catastrophic Landslides and Frontiers of Landslide Science. Springer Nature Switzerland AG, pp 323. https://doi.org/10.1007/978-3-030-60319-9_1)

Sorry, but the discussion about turbulent flow comes later, and I wanted to discuss some other aspects before returning to this topic. **Anyway, removing the part about the alternative rheology brings the discussion about turbulence closer to its first occurrence now (lines 62–79).**

Caption Fig. 2: Thickness depends not only on volume, but also on the confinement conditions and on the distance from the source slope – there are distinct evidence that some laterally confined rock avalanches moved initially as a thick body and then thickness decreased sharply.

Right, but Voellmy's rheology involves thickness, and this is plotted here. **I added a discussion about the change in thickness at the end of Sect. 4 (lines 330–338).**

Line 115: Unclear what you mean.

I added a short explanation (lines 122–123).

Line 129–130: See comment above.

I added a discussion about the change in thickness at the end of Sect. 4 (lines 330–338).

Eq. 9: I would like to see some additional comments on the physical meaning of this parameter. It would be really useful if you will add a list of all parameters used in the equations with all these quantities dimensions and their physical meaning (may be a schematic cross-section). It will help a lot to readers from the geological community to follow your explanations.

I added some more explanation (lines 162–166), but I am afraid understanding the meaning of λ as the fundamental parameter will still be challenging to most of the readers from the geological community. Concerning the other parameters, I hope that the schematic cross section (new Fig. 2) is helpful.

Line 284: Should be specified. Some comments on the confinement will be really useful.

I added a discussion about the change in thickness at the end of Sect. 4 (lines 330–338).

Line 321: See my comment about definitions of parameters

Ok, but it does not make sense to start from zero again in the conclusions.

Line 329: What about same volume in different topographies (different confinement conditions).

Same volume on slopes of different sizes was considered, but different confinement conditions cannot be investigated easily in a lumped-mass model, although probably interesting.

Reviewer 2

Manuscript presents a complementary approach to Voellmy rheology based on the separation of behaviour with respect to flow velocities. The approach proposed is demonstrated with a lumped mass model, of which limitations are clearly stated by the author. As demonstration is limited to a very simple application, discussion is also not presented at depth, which obscures the potential of the approach developed. In order to substantiate the claims made by the author especially in the Conclusions section, further demonstrations using other forms of simplified topographies can be presented.

The paper is designed to present the simplest scenario that explains the observed scaling between $\frac{H}{L}$ and V reasonably well. Of course, it is limited. Going deeper would, however, require the switch to numerical simulations, and then the paper would probably become twice as long.

Even though the manuscript has an unorthodox structure for a scientific paper, it is clearly written and conveys its message to its readers. However, it reads more like a technical note than a research paper at its current state. I suggest reconsideration of the manuscript after major revisions.

I know that equations are less popular than fieldwork photos, and I am aware that not many readers will go through the theory. However, I am part of an older generation of scientists with a rather theoretical background. So I am afraid that I will not be able to write a paper that does not read like a technical note.

Specific comments

1. Page 2 – LL 33-35: The author makes several claims about the mechanism behind the fluid-like behaviour of rock avalanches. This paragraph sounds rather speculative, unless it is supported by references to published works in the literature. I suggest to include citations to convince the readers that there is no consensus on the aforementioned mechanisms.

I added some references (lines 35–38).

2. Page 3 – LL 71-73: The author explains the selection of Voellmy rheology only based on its wide use. If the approach by Jop et al. (2005) is not used elsewhere in the manuscript, what is the motivation of introducing it? Please highlight this.

To be honest, the only motivation was that an editor of "Nature" found this rheology important some years ago. Personally, I do not believe that this rheology works well for snow or rock avalanches, which was also found by Lucas et al. (2014). **So I take the chance to remove the detailed discussion of this rheology (lines 62–79),** which distracted the manuscript a bit. Now the only reference to this rheology is that the problem with the Coulomb friction coefficient is the same as for Voellmy's rheology (lines 102–104).

3. Page 3 – Figure 1: This figure is not clear enough in its current form. Choice of colours hinders differentiating the groups of lines. It might be useful to change line type between Voellmy and Jop et al., and use different shades of a colour within the same group to highlight the thickness used. Readers could also benefit to know about the exact thickness used in the calculations that yielded lines in Figure 1, which can be given with a legend.

I removed the figure since the discussion of this rheology somehow distracted the manuscript.

4. Page 5 – LL 113-115: Could the author suggest even approximate ranges for "high velocities" and "low velocities"?

This is not so easy. If we combine $\xi = 500$ m/s^2 proposed by Voellmy (1955) for snow with a "realistic" value $\mu = 0.75$ for rock, Coulomb friction would still be dominant up to $v \approx 60$ m/s at $h = 10$ m and $\beta = 0$, so practically always. Best-fit values (e.g., Aaron et al. 2022) are, however, rather $\xi \approx 300$ m/s^2 and μ down to about 0.1. Then the v^2-term would already become dominant at $v \approx 17$ m/s, which may practically be achieved. I could add a discussion on this topic, but I am afraid that it will be distracting rather than helpful here.

5. Page 5 – Figure 2: Similar to Comment #2, a legend indicating the thicknesses used in the calculations would be beneficial.

I added a legend and some more explanation (lines 133–139). Hopefully, it is helpful, although I am not completely sure.

6. Page 15 – Line 341: "The new approach should be able to improve numerical continuum simulations". I think this is a strong statement without testing it. I suggest reformulating this sentence.

I rewrote the sentence (lines 369–370).

Technical corrections

I did not detect any obvious typos in the manuscript at its current state.

Community comment 1 (Matthias Rauter)

The manuscript deals with the unsolved problem of extreme runouts of large landslides and rock avalanches. I not necessarily agree with the approach, but it is a pressing issue and interesting viewpoint and deserves more discussion. The figures are great and clean!

I suggest the publication after a major revision.

Major issues:

The scope of the manuscript is a bit unclear to me. Is it to understand and explain the mechanisms in large avalanches? To develop a better friction model? (Then the issue below should be discussed in more depth) Should it be a physically consistent or an empirical approach? The interpretation of λ as a length should then also be discussed in more depth.

It should be clear already from the title that the approach attempts to modify Voellmy's rheology in order to be able to predict the long runout of rock avalanches. It should be physically consistent in some sense (more consistent than the original RKE model), but not be a new microscopic theory. **I added a bit more discussion about the meaning of the length scale λ (lines 162–166).**

I do not understand the scaling (section 4). The runout scales with the friction term but reinterpreting it as a length scale does not seem to be useful to me. I also cannot follow Eq. (24). I also think that physical relations and parameters are mixed together with empirical relations which does not always make sense. I would check this section carefully.

Of course, it makes sense to combine different physical properties to length scales or to time scales. This is the usual way to understand the behavior or models or the respective differential equations. I realize that I lost you quite soon in this section. In turn, however, the theory presented in your own papers is much more complicated than my approach (only ordinary differential equations and exponential functions). So I cannot imagine that the mathematical background is any challenge for you. **Anyway, I added some more explanation to Eq. 25 (previously Eq. 24, lines 251–252)** since the argument behind it seems to be the most challenging part of the scaling stuff. **Furthermore, I mentioned explicitly at which point additional empirical data come into play (lines 310–311).**

The biggest problem I have is the sharp transition between the regimes and the jump in the friction. This seems rather unphysical to me and might lead to severe numerical issues.

I learned parsimony as a fundamental principle when I studied physics in the 1980s. Admittedly, this is long ago, but I still prefer not avoid unnecessary complications of models. So what should be the advantage of softening the transition artificially? The jump in friction also does not cause numerical issues since only the acceleration becomes discontinuous, while the velocity remains continuous. The main effect of the jump in friction (beyond the runout length) is that the deposits may become hummocky, which is not necessarily unrealistic. However, I cannot address numerical aspects in this paper since it is subject of another manuscript (in GMD).

I would also bring in some real case examples. Even using the simple model it can showcase the behaviour of the rheology.

I do not think that it would make sense to apply the simple lumped-mass model to real-world examples.

Minor issues:

I am not sure how strict this journal is with structuring of the manuscript, but the structure is rather unconventional, mixing methods, results and discussion.

I think that it is the only way to make the paper accessible to a non-mathematical community. It looks as if Reviever #2 referred to the "unorthodox structure" and found it ok.

We went trough the same exercise a couple of years ago, deriving a modified Voellmy model from kinetic theory (Rauter et al., 2016). We came to a few different conclusions.

The scope seems to be different to me. You developed a rheology that is somehow similar to Voellmy's rheology and presented a theory that goes much deeper into kinetic theory than the ideas of Salm (1993). However, your result was that it fits for snow avalanches as well as the original Voellmy rheology. I think this is ok for snow avalanches, but it will probably not solve the problem of the long runout of rock avalanches or maybe even fail completely at large thickness.

Line 19: "$H/L < 0.1$, while typical values of μ for Coulomb friction are between 0.5 and 1." It should be described why and how these are connected/correlated.

It is explained in the previous sentence. However, it would not be very convenient to derive the relation mathematically in the beginning of the introduction. So I referred to the textbook of de Blasio (2011).

Line 34: "Water is present in many rock avalanches and may play a part as well as air. Frictional heating may also have a strong effect on the mechanical properties. Alternatively, the increase in runout length with volume may be an inherent property of granular flow without any specific process beyond the interaction of particles." This needs to be backed up with references. I also suggest to look at Kesseler et al. (2020) in this context.

I added some references (lines 35–38).

Line 46: "The most widely used relation for the basal shear stress". The role of the basal shear stress should be explained. How is it connected with μ and H/L?

I am afraid that I did not get your point. All these depth-averaged models use a shear stress at the bed.

Line 150: "More important, however, it defines the length scale of adjustment of the velocity to the slope." I do not agree with this statement. A length scale would be the typical height H or length L. I also do not understand "adjustment of the velocity to the slope". Is it the slope length at which terminal velocity is obtained?

No, the terminal velocity is never reached since the kinetic energy approaches its asymptotic value exponential. **I added some more explanation (lines 162–166).** And of course, length scales are not necessarily geometrical properties, but can also be combinations of physical parameters.

Line 156: "the slope length l" I would prefer a large L, since this is usually a rough scale like as in line 19. If it is not, it deserves an explanation. Generally, I would distinguish clearer between scales and real distances.

I would also prefer an uppercase L, but this symbol is typically occupied by the horizontal runout length (not only in other papers, but also here).

Line 165: "phase space trajectories". This term is new for me for this kind of diagram.

In mechanics, phase space diagrams are typically momentum vs. position, but velocity is equivalent to momentum.

Line 188: "The relation to the RKE model" This model has (as most models) some issues (Issler et al. 2018). I would make sure that they do not change your conclusions.

As the criticism of Issler et al. (2018) was the starting point for reinterpreting the RKE concept, I have no idea how it should affect the conclusions.

Line 235: "Figure 5(a) shows the dependence of S/l on λ/l" The runout scales with the friction coefficient. That seems obvious. Why the division with l?

I think it is explained directly in the previous paragraph. Eq. 23 (formerly Eq. 22) contains λ and $\frac{\lambda}{l}$, and dividing both sides by l is the only way to express everything in terms of $\frac{\lambda}{l}$ and ϵ.

Line 241: "The existence of two different scaling regimes" I am not sure about this. Are there really two regimes? In the following section you look only at very extreme scenarios. This is hard to say without some real world examples.

Ok, I reformulated it in order to clarify that the scaling properties hold for the end-members and that the subdivision at $\frac{\lambda}{l} = 0.8$ is just a definition that makes sense (lines 258–261).

Line 244: "For $\lambda \ll l$, Eq. (18) yields" So basically an infinite slope?

You can interpret it in terms of l or λ. It is either an infinite slope or high friction, so the situation in which we come close to the terminal velocity before reaching the foot of the slope. This is why considering the ratio $\frac{\lambda}{l}$ makes sense.

Line 247: "$\lambda \gg l$" Basically frictionless?

Either small friction or a long slope, so that the mass is still accelerating at the foot of the slope.

Line "the runout length increases with increasing slope length" Are they not the same or different by a factor $\cos\beta$ at most? I wonder if we are turning in a circle by multiplying all kind of relations.

Of course, not. The slope length l is a property of the considered topography, while the runout length (either S or L, depending on the coordinates) is a property of the landslide (so where it stops). I hope this is clearer with the new Fig. 2.

Line 289: "L/H mostly decreases with l/λ" If $l = L$ then this would just mean λ decreases with H?

Formally yes, but the runout length L is the obtained property, while H, l, and λ are parameters. So you would compare landslides that reach the same runout length at the same slope length. If you then make the slope steeper (increase H), you would need higher friction (decrease λ) in order not to increase the runout length. More or less obvious, but not very useful to investigate.

Line 289: "So the ratio L/H decreases with increasing slope length" Isn't L the slope length?

No, it is the runout length.